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ORIGINAL LECTURES.

THE CARTWRIGHT LECTURES. ON THE GENERAL PATHOLOGY OF FEVER.

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LECTURE I.

THE NATURE OF FEVER.

THERE is no subject in medicine of more general and varied interest than fever. The practitioner in every department of medicine, the pathologist and the physiologist are equally interested in the investigation of the nature and effects of fever. Even the physicist and the chemist, who are not directly concerned with medical science, have lent their aid to the study of animal heat and its disorders. The history of opinion regarding fever is in great part the history of medicine itself, for no feature of the great systems of medicine from Hippocrates and Galen to the present century so characterizes these systems as the views held concerning the nature of fever. In consequence of the importance of the subject and of the number and ability of those engaged in its investigation, it might be supposed that no chapter in medical science would be better understood than that pertaining to fever. That such is not the case is due to the fact which is becoming more and more evident that the reaction of the animal system which we call fever is dependent upon the most fundamental and essential properties of protoplasm and of nerve energy. In proportion as our knowledge of these properties increases and becomes more accurate, we gain a clearer insight into the complicated processes involved in the production of fever.

I should hardly have selected for this course of lectures a subject where so many problems remain unsolved and which must necessarily be presented in so fragmentary a form, were it not that in all ages the opinions held concerning the nature of fever have controlled measures employed in its treatment. In proof of this, one need not go back to the time when fever was regarded as an almost conscious struggle of an anima with a noxious principle, in which struggle the physician was to interfere as little as possible, or to the time when fever was supposed to result from morbid humors which the physician should aim to eliminate by the production of a critical discharge, or to the period when the treatment hinged upon the belief either in the sthenic or the asthenic nature of fever. In our own time the treatment of fever is intimately connected with the answers variously given to such questions as whether fever aids in the elimination or destruction of infectious agents concerned in its production; whether increased waste of tissue is a constant

condition and a source of danger in fever; what part is played by infection and what part by elevation of temperature in causing the grave symptoms of fever; what in addition to lowering of temperature are the effects of so-called antipyretic measures of treatment?

I need hardly say that the subject of these lectures relates to fever as a condition common to all febrile diseases. Some writers understand by the term fever used in this sense merely abnormal elevation of temperature, others elevation of temperature and the symptoms directly caused by this, and still others a complex of symptoms of which increased temperature is the most prominent but not necessarily the cause of the others. In considering the general pathology of fever it is convenient to adopt the last meaning, although it would doubtless be less confusing if the word fever were applied only to abnormal elevation of temperature.

Increased temperature being the dominant and essential symptom of fever, all discussions as to the nature of fever centre around the question, How is the febrile rise of temperature produced? It is to the consideration of this question that I first invite your attention. As there are other aspects of fever which I wish to discuss, it will be necessary to present the matter belonging to this division of the subject in as succinct a form as is compatible with clearness. Twelve years ago Burdon-Sanderson¹ brought together in an admirable critical review the results of investigations upon this subject up to that period. Since that time important additions have been made to our knowledge of the mode of production of fever.

In the wonderful preservation of a nearly constant temperature which characterizes in health the warm-blooded animals three factors are concerned, viz., the production of heat within the body, the loss of heat from the body, and the regulating mechanism by which the varying heat production and heat loss are so balanced that the internal temperature remains practically constant. It is theoretically possible that the rise of temperature in fever may be due to the disturbance of any one or more of these factors. It becomes necessary, therefore, to consider the behavior of heat production, of heat loss, and of the regulating mechanism in fever.

We will begin with the consideration of the production of heat in fever. The amount of heat produced by the body is estimated by two methods, one known as direct calorimetry, the other as indirect calorimetry. In the method of direct calorimetry the animal is placed in a closed ventilated box surrounded on all sides by a compartment containing water or air, and the amount of heat discharged from the body is determined by the quantity of heat imparted to the surrounding water or air. If the temperature of the animal remain unchanged during the period of observation, the heat production is equal to the heat loss; if the temperature rise or fall, the amount of heat corresponding to this change

¹ Burdon-Sanderson: *The Practitioner*, 1876.

of temperature—an amount determined by multiplying the weight of the animal by its specific heat and by the number of degrees of altered temperature—is added to or subtracted from the quantity of heat imparted to the calorimeter. Time will not permit me to enter into experimental details in this connection; it must suffice to say that the method of direct calorimetry necessitates the introduction of a number of corrections which cannot be determined with absolute accuracy, so that the results obtained are of relative rather than of absolute value.

Hitherto the estimation of heat production in fever by determining the entire amount of heat liberated from the body has been made only upon animals in which fever has been artificially produced. The most elaborate researches of this nature are those of Senator¹ and of Wood.² The experiments of Wood are of the greater value because he extended his observations over longer periods of time.

In four of the seven calorimetric experiments of Wood on different febrile dogs comparison can be made of the amount of heat produced hourly in fever with that produced by the same animal when fed and when in a state of hunger. During the period of fever the animal was also in a condition of hunger. I have computed from Wood's tables that the average hourly heat production during seven days described as first and second fever days, is a little over 23 per cent. greater than that of the healthy animal during a state of hunger, the minimum excess being 1 per cent. and the maximum 55.5 per cent.³ If a comparison be made of different periods during the existence of fever there are found to be even greater fluctuations in the amount of heat production than these figures would indicate, this amount being sometimes more than double that in hunger and sometimes considerably less than the average production in hunger. Moreover, these experiments show no definite relation between the height of the temperature and the amount of heat produced, nor is it possible to deduce from them any relation between heat production and the different stages of fever, such as the more recent calorimetric experiments of Wood, Reichert, and Hare⁴ appear to show and which will be described later. An important outcome of these as well as of all similar calorimetric experiments is that, although the heat production of an animal in fever is greater than that under like conditions of nourishment, it is, as a rule, less than that of the same animal upon a full diet.

There are many reasons which make it important to control the experimental results obtained from animals in fever by corresponding observations of human beings. The agents used in producing experimental fever have been generally putrid fluids or pus, the injection of which causes sometimes diminution instead of elevation of temperature. When fever is thus produced, it is usually of short duration and of only moderate intensity, the rise of temperature being rarely more than four degrees, and sometimes not more than one degree Fahren-

heit. Moreover, a large part of the important role played by the skin in the regulation of the bodily temperature in man is assumed by the lungs in these animals.

Complete calorimetric observations of human beings in fever encounter difficulties which have not yet been overcome. The imperfect or incomplete methods employed by Liebermeister¹ and by Leyden² in determining the heat production of human beings in fever justify the inference that this production is increased, and, apparently, as a rule, to a greater extent than in animals, but they do not warrant positive conclusions as to the quantity of heat produced.

We turn now to the results regarding febrile production of heat obtained by what has sometimes been called indirect calorimetry. Inasmuch as the heat energy of the body is the result of chemical changes of its proteids, fats, and carbohydrates, it is evident that if we know the kind and the amount and the heat value of the substances consumed in a given time within the body, we can compute their heat production.³ These chemical changes, so far as their final products are concerned, are processes of oxidation. The heat values of the substances consumed in the body were determined first by Frankland and more recently with accuracy by von Rechenberg, Danilewsky, and Rubner. The investigations of Pettenkofer and Voit have shown that in hunger almost exclusively fats and proteids are oxidized, and that in this condition by determining the amount of oxygen absorbed and of nitrogen and of carbon excreted, we can estimate the quantity of fat and of proteid substances oxidized during the period of observation.

Physicians of past centuries regarded increased consumption of the material of the body in fever as so evident that it needed no especial demonstration, and after Lavoisier made apparent the sources of animal heat, it was accepted almost unquestioningly until the last quarter of a century that fever is essentially a process of increased combustion or oxidation. The immense historical importance of the promulgation in 1863 of Traube's theory, which denied the dependence of fever upon increased production of heat, is that this theory has led to a careful inquiry into the grounds of beliefs hitherto generally accepted, and to the demonstration of the unsatisfactory nature of the evidence hitherto thought to be conclusive.

The striking loss of weight of most fever patients is, as is well known, a fact of great clinical importance. Weber and Finkler have demonstrated that animals in fever lose weight more rapidly than healthy animals in hunger, and although observations on human beings with reference to this point are not concordant in their results, there can be little doubt that the tendency of fever is to cause a greater loss of weight than can be explained simply by insufficient nutriment. This tendency, however, may be masked by the retention of water within the body as the investigations of Leyden and others have

¹ Senator: Untersuchungen über d. Fieberhaften Process, Berlin, 1873.

² Wood: Fever, A Study in Morbid and Normal Physiology, Philadelphia, 1880.

³ In making this computation I have corrected some numerical errors in Wood's tables; these errors are not serious and do not affect his conclusions.

⁴ Wood, Reichert, and Hare: Therapeutic Gazette, 1886.

¹ Liebermeister: Handb. d. Path. u. Therap. d. Fiebers. Leipzig, 1875.

² Leyden: Deutsches Arch. f. klin. Med., Bd. 5.

³ The energy resulting from these chemical changes appears partly in the form of heat and partly in mechanical work. As practically all of the internal mechanical work is transformed within the body into heat energy, it is only the external mechanical work which is to be considered in estimating the actual heat production. The influence of this factor in fever will be spoken of later.

shown. While, therefore, the studies of the loss of weight in fever leave no doubt that there is increased destruction of tissue in this condition, we cannot consider this loss as an accurate measure of the increased destruction, nor, without further knowledge, as an indication of increased oxidation, still less as proof of excessive production of heat.

It was for a long time believed that the excessive excretion of urea in fever afforded satisfactory evidence of increased oxidation and of greater production of heat. We now know, especially from the researches of A. Fränkel,¹ that this is so far from being true that we could explain the excessive elimination of urea better upon the assumption of diminished than of increased oxidation of tissue. In certain pathological conditions, notably phosphorus poisoning, the amount of urea excreted may be increased more than threefold, notwithstanding, or, as Fränkel believes, in consequence of diminished absorption of oxygen and elimination of carbonic acid. It can, moreover, be computed that even without any diminution of the respiratory gases the discharge of urea may be increased without greater production of heat. While, therefore, the enormous increase in the discharge of urea in fever sheds valuable light upon a most obscure subject, the nature of febrile metabolism, it does not, regarded by itself, afford us any information as to the production of heat.

Failing to find satisfactory proof of increased oxidation in the loss of weight of the body, or the excessive excretion of urea in fever, attention was then directed to the elimination of carbonic acid, an excretory product which bears a much closer relation to the production of heat than does urea. Immense importance has been justly attached to the determination of the amount of carbonic acid excreted by an individual in fever. No point in the whole battle-ground of fever pathology has been more hotly contested than whether increased production of carbonic acid is an essential part of the febrile process. The first investigators of this question, Leyden, Liebermeister, Senator, contented themselves with the determination of the amount of carbonic acid eliminated by an individual in fever and in apyrexia. There are several considerations which greatly diminish the value to be attached to the mere estimation of carbonic acid excreted without simultaneous determination of the amount of oxygen absorbed. As has been urged by Senator, and with especial clearness by Pflüger, increased discharge of carbonic acid does not necessarily imply increased production of the same. The discharge of carbonic acid varies, independently of its production, with the rhythm and depth of respiration, and with the temperature and the alkalinity of the blood, all factors which are altered in fever in such a manner as to favor increased liberation of carbonic acid. It is true that the influence of these factors would cause increased discharge of carbonic acid out of proportion to its production only for a limited period, and that the prolonged increase in the amount of carbonic acid discharged in fever, which has been found by several observers, can hardly be interpreted otherwise than in favor of increased production. There is another point which detracts still further from the value of exclusive determinations of the quantity of carbonic acid discharged, and this is that it makes a great

difference, so far as the production of heat is concerned, whether the carbonic acid is the result of oxidation of carbohydrates, of fats, or of proteids, a difference amounting, according to Rubner,² to 29.5 per cent.; or, if only the proteids and fats be considered, to over 20 per cent. In investigations of nutrition it is now known to be of the utmost importance to determine the so-called respiratory quotient—that is, the ratio between the amount of carbonic acid discharged and that of oxygen absorbed. This quotient varies in a definite way with the kind of material oxidized in the body, and an accurate knowledge of it would enable us to draw conclusions as to the substances consumed in fever.

The investigations which have been published within the last few years upon the absorption of oxygen, as well as the discharge of carbonic acid in fever, are to be ranked as most valuable contributions to our knowledge of the subject. The first determination by trustworthy methods of the amount of oxygen absorbed and of carbonic acid excreted in fever was made in Pflüger's laboratory by Colasanti upon a guinea-pig, and was published in 1877. Since that time two careful and elaborate researches upon this subject have been made, the one by Finkler, and the other by Lilienfeld.³

These experimenters found that in fever there is increase of the amount, both of oxygen absorbed and of carbonic acid excreted. Making comparison with healthy animals under the same conditions of nutrition, Colasanti found that the increase in the absorption of oxygen amounted to 18 per cent, and in the excretion of carbonic acid to 24 per cent.; Finkler, whose experiments were made also upon guinea-pigs, found, under varying conditions of external temperature, the average febrile increase of oxygen to be 13.8 per cent., and of carbonic acid 15.3 per cent., and Lilienfeld, who experimented on rabbits, found the average increase of oxygen to be 13.9 per cent. The statement of these averages gives an incomplete conception of the oxidation in fever, as they are derived from all stages of fever, and varying elevations of temperature. No constant proportion was found to exist between the height of the temperature and the amount of oxidation. On the other hand, a relation was observed between the oxidation and the stages of fever, viz., the initial stage with rising temperature, the acme with constant high temperature, and the defervescence with falling temperature. During the period of rising temperature oxidation was increased, and in this stage Finkler found the highest percentages, amounting to 36.6 per cent. in the increase of oxygen, and 37.6 of carbonic acid. There were, however, marked fluctuations in this stage, both in the temperature and the amount of oxidation. In the stage of constant high temperature such high percentages were not noticed, and the fluctuations were less marked. The processes of oxidation, according to Lilienfeld, are increased, on the average, less in the acme than in the initial stage of fever.³ During defervescence of fever the increased oxidation falls, and may sink below the normal. Taking a broad view of these stages, we may say, therefore, that the periods of rising,

¹ Rubner: *Zeitschrift f. Biologie*, Bd. xxi.

² Colasanti, Pflüger's Archiv, Bd. xiv. Finkler, *Ibid.*, Bd. xxix. Lilienfeld, *Ibid.*, Bd. xxxii.

³ In the period of rising temperature there was an increase of oxygen absorbed of 27 per cent., in the acme of 14.9 per cent.

¹ Fränkel: *Virchow's Archiv*, Bd. 67.

constant, and falling temperatures in fever, correspond to periods of rising, constant, and falling oxidation, but we must bear in mind that the fluctuations in oxidation are much greater than, and bear no constant relation to, those of temperature; so that, in each period, there are times when oxidation may rise or fall most decidedly without corresponding changes of temperature.

Inasmuch as these experiments have shown that the increased excretion of carbonic acid in fever is accompanied by increased absorption of oxygen, and, as will be explained presently, that the respiratory quotient, if it changes at all, sinks, it is evident that we need not discard experiments in which only the amount of carbonic acid excreted has been estimated by good methods. The most accurate of these experiments are those of Leyden and Fränkel upon fevered dogs.¹ They found that, without exception, carbonic acid is excreted in larger amount in fever than in health under the same nutritive conditions, the excess amounting sometimes to 70 per cent., and in general being larger than in the experiments which have been previously considered. The earlier and meritorious experiments of Senator upon this point did not give uniform results, and he felt justified in concluding that there is no evidence of increased production of carbonic acid in fever. Subsequent experiments with far more accurate methods have demonstrated the incorrectness of Senator's conclusions. It may be well to call to mind that Burdon-Sanderson's deductions, which have naturally had great influence among physicians here and abroad, were based, in great part, upon the data derived from Senator's experiments.

Hitherto, the methods employed in studying the respiratory gases of human beings in fever have not approached in accuracy those used in the experiments described. If the discordant results of Wertheim, which were obtained by methods manifestly very inaccurate, be discarded, all other investigators have observed augmented discharge of carbonic acid in fever of human beings. Leyden found an excess of 50 per cent. in the febrile discharge of carbonic acid; and Liebermeister, whose observations were made chiefly on cases of intermittent fever, found an excess of 30 to 40 per cent. in the period with rising temperature, and of 19 to 31 per cent. in the acme of the fever. During the defervescence of the fever the excess of carbonic acid discharged diminished, and sometimes wholly disappeared. In one instance, in which the determination was made during the rigor of intermittent fever, the carbonic acid excreted was two and a half times the normal amount; an excess so enormous that it was undoubtedly due, in great part, to the muscular movements which attended the chill.

Although we cannot consider these figures as absolutely accurate, they indicate clearly that in human beings, as well as in animals, fever is characterized by increased oxidation, and apparently that, as a rule, in man the excess of oxidation is greater than in the experimental fever of animals. This was to be expected, as it is difficult to produce experimentally in animals anything approaching in intensity the well-marked fevers of human beings.

As the result of these laborious researches we may consider it established that increased oxidation is a part of the fever process. It has been claimed that this aug-

mented oxidation is simply the result of the elevation of temperature, but it can be proven that this is not true. Pflüger has demonstrated that the processes of oxidation are more active at high than at low temperatures of the body, and he has also established the increment of oxidation which corresponds to each degree of rise of temperature. By means of these data Finkler has computed that in guinea-pigs the febrile elevation of temperature of 1° C. could cause an increase of the absorption of oxygen of only 3.3 per cent. Moreover, Lilienfeld found decided increase in the processes of oxidation before there is any marked elevation of temperature, and all the experiments have rendered it quite evident that there is no such relation in fever between the height of the temperature and the energy of oxidation, as would be expected if the augmented oxidation were merely the result of the increased temperature.

There is no reasonable doubt that the more energetic oxidation which we find to be an essential part of the process of fever indicates increased production of heat. Exactly what amount of heat production corresponds to the increased oxidation we cannot know until the kind and the quantity of substances oxidized in fever have been determined. It is to be regretted that no experiments have been made in which the amount of nitrogen excreted has been determined at the same time with the estimation of the oxygen absorbed, and of the carbonic acid discharged. These data would enable us to form some estimate, although not an accurate one, of the amount of heat production corresponding to the oxygen absorbed, unless very different laws from those in health control the oxidation processes of fever.

Mention has already been made of the importance of determining in fever the respiratory quotient, or the ratio between the carbonic acid discharged and the oxygen consumed. A few words will make this clear. It is well known that under ordinary circumstances in health not all of the oxygen consumed reappears in the carbonic acid discharged. This indicates that a part of the oxygen absorbed is used in other oxidations than those resulting in the production of carbonic acid. According to the extent of these other oxidations, therefore, the respiratory quotient must vary. It is probable that these oxidations, of which carbonic acid is not a product, result at least in part, in the formation of water, which is, therefore, one of the excretory products of the body, as has been urged especially by Austin Flint. The influence of various circumstances upon the respiratory quotient has been studied, but what especially concerns us here is that in hunger this quotient sinks, which is to be expected from the fact that in this condition almost exclusively fats and proteids are oxidized. We evidently possess in the determination of the ratio of carbonic acid discharged to the oxygen consumed a means of reaching a conclusion as to a cardinal point in the pathology of fever, viz., whether the processes of oxidation in fever conform to the laws which govern them in health, and particularly whether, as has been often asserted, unusual or incomplete products of oxidation are formed to any considerable extent in fever. Colasanti and Lilienfeld found that the respiratory quotient in their fevered animals did not vary from that of healthy animals under similar nutritive conditions. Finkler observed that the respiratory quotient fell in fever somewhat more rapidly than in hunger, and this he explains by the more active oxidation in fever.

¹ Leyden and Fränkel, Virchow's Archiv, Bd. 76.

All three experimenters reached the conclusion that the substances oxidized are the same in fever as in health, and that other than the normal products of combustion are not formed in fever in any considerable amount. That the metabolism in fever does differ in at least one important respect from that in hunger, is evident from the excessive amount of urea excreted in fever, but considerable variations in the disintegration of albuminous material may occur without much change in the respiratory quotient.

The only determinations of the febrile consumption of oxygen and discharge of carbonic acid in man are those of Wertheim and of Regnard. Both investigators found a decided diminution of the respiratory quotient. The method employed by Wertheim was so defective that no confidence can be placed in his results. The experiments of Regnard¹ are presented with great neatness, but his results on other points differ so much from those obtained by trustworthy physiologists and by better methods, that we cannot accept his conclusions as to the respiratory quotient in fever without confirmation. Regnard found in all fevers which he studied a most marked diminution of the respiratory quotient. If this were true it would follow that in fever a much larger part than in health of the oxygen consumed is employed in other oxidations than those yielding carbonic acid. This would confirm the widely accepted belief that water and products of incomplete oxidation are formed in excessive amount in fever.

From the unfortunate discrepancy of these results it is evident that the knowledge which we now possess of the processes of oxidation in fever is not sufficient to enable us to form from them an accurate estimate of the heat production. In hunger, from one-fourth to one-third of the absorbed oxygen is available for combination with hydrogen to form water, the remainder uniting with carbon to form carbonic acid (Regnault and Reiset). If in fever the same ratio exists, then the excess of heat production would be proportionate to the excess of oxygen absorbed, assuming that the substances oxidized are the same in both conditions; if, however, as Regnard's experiments indicate, a larger proportion of oxygen is available in fever for the oxidation of hydrogen, then the increment of heat production would be still greater, for the same amount of oxygen produces more heat when employed in the oxidation of hydrogen than in that of carbon. It is also to be considered that the same quantities of hydrogen and of carbon in their oxidation yield varying amounts of heat according to the chemical compounds in which they are contained, and we cannot say positively whether the compounds oxidized are the same in fever as in health under like conditions of nutrition. Upon the whole the weight of evidence is in favor of the view that the excess of heat production in fever is approximately proportionate to the increase in the consumption of oxygen, but it would be rash to assert this positively. It is evident that in fever ordinarily a much smaller amount than in health of the energy resulting from chemical processes is transformed into external mechanical work, so that relatively more remains in the form of heat.

In connection with this discussion of the possibility of unusual sources of heat in fever may be mentioned an

hypothesis which has been advanced with much ingenuity by Dr. Ord.¹ This hypothesis is based upon the assumption, which is probable enough, that there are in the body chemical processes in which heat energy is transformed or rendered latent. These processes are thought to be chiefly those concerned in the building up of tissue. It is argued that inasmuch as the construction of tissue is manifestly in abeyance in fever, the amount of heat in the body may be increased not only by disintegrative processes, but also by "the persistence in the form of heat of energy which should have taken another form." That these building up processes influence decidedly the amount of heat produced in the developing ovum has been demonstrated by d'Arsonval's calorimetric determination that the egg during incubation absorbs heat, notwithstanding the consumption of oxygen and the excretion of carbonic acid. This fact, which might have been predicted, certainly does not justify us in refusing to attach any calorimetric value to the determination of the respiratory gases and the urinary nitrogen. From the little we know of these constructive processes in human beings we should infer that the amount of energy in the form of heat which they appropriate bears only a very small ratio to the total heat energy set free by heat-producing processes, so that their cessation would not bring a large increment to the heat of the body. Moreover, these constructive processes are also in abeyance, although doubtless to a less extent, in starvation, with which experimental fever is usually contrasted as regards heat production and heat loss. It is not probable that any extraordinary difference in the behavior of the processes of tissue-building in fever and in starvation can occur without affecting the respiratory quotient. For the present, therefore, we cannot attach any great importance, so far as the increase of heat energy in fever is concerned to the inactivity of heat-absorbing processes.

We have gone over now the evidence which, in my judgment, establishes the fact that there is increased production of heat in fever. The same conclusion is reached also by the study of the loss of heat from the body in fever. That fever is accompanied by increased production of heat and by more active combustion, has been in all ages the belief of the majority of physicians. This belief, however, has been rather instinctive than based upon actual demonstration. It has been contested by investigators of great ability, and on the ground of scientific observation. For these reasons, and on account of the importance of the subject, it has seemed to me desirable to present to you the exact evidence, although many of its details, I fear, may have wearied you. We have learned, moreover, certain facts concerning febrile thermogenesis which the mere observation of fever patients does not render so apparent. We have found that there is no definite relation between heat production and the height of the temperature, so that we may have excessive thermogenesis with low as well as with high temperatures. There appears to be, however, a relation between the stages of fever and heat production, this being in spite of remarkable fluctuations greatest in the initial stage, and least in defervescence.

Although for reasons which have been mentioned, and others might have been adduced, we can attach hardly an approximative value to figures which purport to give

¹ Regnard: *Recherches Exp. sur les Variations Pathologiques des Combustions Respiratoires*. Paris, 1878.

¹ Ord: *British Medical Journal*, 1885, vol. ii.

the actual heat production in fever, still, unless far more serious errors than seems possible inhere in the methods of direct and of indirect calorimetry, we can draw one important conclusion. This is that while an individual in fever produces more heat than he would in health under similar conditions as to food and muscular movements, he does not produce necessarily in fever more heat than he would in health on a full diet. And it is certain that he usually produces far less heat in fever than he often does under circumstances which normally increase heat production, such as a cool environment and muscular exercise. That one in health, with little or no change of temperature, may produce twice or more the quantity of heat which he produces in fever, makes it plain that it is impossible to explain febrile rise of temperature simply on the basis of increased thermogenesis, or what probably comes to the same thing, of increased oxidation. That in health vastly increased heat production may occur with comparatively little change of temperature is, of course, due to the fact that the dissipation of heat is proportionately increased. It is self-evident, and, so far as I know, has never been disputed that in fever the equilibrium is so disturbed that heat loss does not correspond to heat production as it should in health. This disturbance of equilibrium can be brought about in various ways, and it is only by the determination of the actual heat production and heat loss in fever that we can say in what direction the balance is disturbed.

We have found that the production of heat is increased in fever when the comparison is made with like conditions of nourishment and of environment. It is obvious that the total loss of heat cannot equal the total production of heat during the period of febrile rise of temperature.

As is well known, most of the heat of the body is liberated from the skin and from the lungs; from the former by radiation and conduction and by the evaporation of moisture, and from the latter by evaporation of moisture and warming the respired air. It has been estimated that in man about eighty per cent. of the total heat dissipation is from the skin.

The method of direct calorimetry, already described, has been applied only to animals for the determination of the total heat loss in fever. Here Wood's experiments are the best which we possess. Leyden and Liebermeister have furnished calorimetric data which, although unsatisfactory in many respects, indicate the general direction of febrile heat loss in human beings. All of these experiments show that more heat is dissipated in fever than under like conditions in health. The fluctuations of heat loss during a febrile paroxysm are so great that the statement of an average for the entire period has little significance. Such an average, according to Wood's results on febrile dogs, would fall between twenty to thirty per cent. excess of heat loss as compared with the loss in health under like conditions of food. The dissipation of heat in fever, however, may be at times more than double the normal amount, and again may sink below the norm. We have not sufficiently accurate estimates either of the total amount of heat produced or of that dissipated during a febrile attack to enable us to strike a balance between the two. Some persons have been so impressed with the large amount of heat lost during certain periods of fever, that they have concluded that there must be far greater excess of heat production than previous investigations

have shown to be probable. They suggest that there are sources of febrile heat of which at present we have no idea. Such conclusions seem to me quite unwarranted, when we consider the behavior of heat-loss, not for a limited time but throughout the different stages of a paroxysm of fever. We have already seen that we obtain no satisfactory conception of febrile production of heat unless we follow it during the stages of fever, and this is no less true of heat dissipation. The observation of the condition of the skin as regards temperature and moisture must in all ages have afforded an insight into the general behavior of heat dissipation during the different periods of fever. It does not require any instruments of precision to make plain the fact that a cold, dry skin, such as we observe during a febrile chill, liberates less heat than normal, or that a hot, moist skin, such as we are likely to find at the defervescence, loses more heat than normal. Not quite so evident is the direction of heat loss during the hot stage or acme of a febrile paroxysm or fastigium of a continued fever. Here the skin usually appears hot and dry. The ordinary impression that more heat than normal is dissipated during this stage is supported by calorimetric experiments. There can be no doubt that the elevation of the cutaneous temperature which we observe in the hot stage of fever causes an increase in the amount of heat lost by radiation and conduction. Similar elevations of cutaneous temperature in health, such as those caused by muscular exercise, are accompanied by increased moisture of the surface. Not only is visible perspiration usually absent during the hot stage of fever, but the invisible perspiration is, as a rule, relatively although not absolutely diminished, as Leyden has shown. The dryness of the skin, therefore, is a factor which in the hot stage of a fever tends to lessen heat dissipation. Clinical observation, however, shows that fevers differ markedly from each other as regards perspiration during the hot stage, the skin being sometimes bathed in perspiration without any depression of temperature. It would undoubtedly be of great interest to possess trustworthy data as to the exact loss of water from the surface of the body in different fevers and at different stages of fever. We cannot place much reliance upon the indications afforded by Weyrich's hygrometer, which has been repeatedly used for this purpose. More is to be expected from the method employed by Peiper¹ in studying insensible perspiration under physiological conditions.

The general impressions regarding febrile loss of heat derived from clinical observations, are supported by calorimetric experiments. The dissipation of heat is least during the initial stage of fever, and greatest during the period of defervescence. During the hot stage or fastigium heat dissipation exceeds the normal, but usually, on account of the dryness of the skin, not so much as one might infer simply from the impression of heat received by the hand when placed upon the skin.

During the initial period the loss of heat, although on the average less than in the following stages, is usually greater than normal. If, however, the rise of temperature be rapid the heat loss falls below the normal amount. As there is now increased production of heat, there is evidently a glaring disproportion between the two factors, heat production and heat loss, and under these circumstances the febrile attack is likely to be ushered in by a chill.

¹ Peiper: *Zeitschr. f. klin. Med.*, Bd. 12.

During the febrile chill all the efforts of nature combine to produce in the shortest time the greatest possible elevation of temperature. Not only is heat loss reduced to a minimum, but heat production is excited to the utmost.

During the period of defervescence, on the other hand, the relation between heat loss and heat production is reversed. This is, of course, most apparent when the fever terminates by crisis with rapid fall of temperature. Then the loss of heat is excessive, being sometimes threefold that in the normal state, and the production is relatively and often absolutely diminished.

It is of importance to remember that there are continual and irregular fluctuations in the dissipation of heat during the different stages of fever. These fluctuations bear no definite relation either to the momentary production of heat, or to the height of the internal temperature. We deal in ordinary life so much more with units of temperature than with units of heat that it is difficult for us to keep constantly in mind the fact that no inference can be drawn as to the height of the internal temperature from the knowledge of the momentary heat production and heat loss. If the heat loss falls behind heat production the temperature of the body rises, and it can remain at this elevated point with either diminished or increased heat production so long as the heat loss equals heat production.

No correct conception of the condition of the heat-regulating mechanism in fever can be obtained without taking into consideration these irregular variations in the discharge of heat, and it is a merit of Senator and his pupils to have emphasized particularly this point. These variations are made apparent not only by calorimetric measurements and direct observation, but also by the studies which have been made of the cutaneous temperature in comparison with the internal temperature in fever. Hankel's law that the difference between the temperature in the axilla and that of the surface of the body in the febrile condition is less than in the normal condition manifestly does not hold for the chill, in which it has been proven that the superficial temperature falls while the internal temperature rises. Jacobson, Wegscheider, Schülein,¹ find that there constantly occur in the course of most fevers changes of the superficial temperature, which stand in no relation whatever to alterations of the internal temperature. Schülein thinks that he has discovered some facts in this regard which are available in diagnosis. However this may be, these observations indicate that contraction and dilatation of the cutaneous vessels are constantly occurring, and without any regularity, in fever. These irregular variations in the calibre of the bloodvessels are also apparent to the eye in the vessels of the rabbit's ear during fever. When it is considered, furthermore, that these irregularities of cutaneous circulation vary decidedly in different situations, no further proof is needed that the mechanism which regulates the discharge of heat from the surface of the body is profoundly disturbed in fever.

These alterations in cutaneous circulation are such striking phenomena that it is perhaps not surprising that two great medical authorities should have based upon them exclusively theories of fever, Traube assuming excitation and Marey paralysis of vaso-motor nerves as

the primal element in fever. We need not discuss these theories, now generally abandoned in their exclusive form. From what has been said concerning the loss of heat in fever, it is evident that we cannot explain febrile rise of temperature solely by the behavior of heat dissipation any more than we can explain it solely on the basis of increased heat production. In rejecting Traube's theory that fever is the result solely of retention of heat we must still recognize the fact that decrease in the dissipation of heat, at times absolute, at other periods relative, is a factor of the utmost importance in the febrile process.

From whatever point of view we consider the question we cannot avoid the conclusion that it is the mechanism which controls the relation of heat production to heat loss, which is disturbed in fever.

Heat production is increased in fever, but if the regulating mechanism were normal, then the discharge of heat would be proportionately increased and the temperature would not be materially affected. Nor would the force of this argument be changed if febrile thermogenesis were twice as energetic as we suppose it to be.

The loss of heat is increased in fever, so that on this ground alone we should be obliged to assume increased heat production. But, even if it were proven that heat dissipation is diminished, as has been recently claimed again by Rosenthal,¹ and that fever is the result of heat retention alone, it would still be necessary to admit that the regulating mechanism is at fault, for Pflüger has demonstrated that when this is normal, changes in the temperature of the skin are attended by such changes in heat production that the internal temperature remains within wide limits unaltered.

I repeat then that the conclusion is forced upon us that the fever-producing agents must act either directly or indirectly upon the mechanism regulating the harmonious relation of heat loss to heat production.

(To be concluded.)

ORIGINAL ARTICLES.

A CASE OF RADICAL CURE OF HYDROCELE BY EXCISION OF THE TUNICA VAGINALIS TESTIS.²

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A MAN, æt. sixty-three, was admitted to St. Mary's Hospital, December 17, 1887, with a hydrocele of three years' standing. The tumor was as large as a good-sized orange. For the following notes of the case I am indebted to Dr. J. C. Heisler, surgical interne.

December 20, 1887. An incision was made the whole length of the tumor. The hemorrhage from the scrotum was quite free, but was easily controlled by hemostatic forceps, and eventually no vessel required ligation. The sac was then incised, when its liquid contents at once escaped, and the edges of the sac were seized on each side by hemostatic forceps. By means of the fingers and the closed points

¹ Jacobson: Virchow's Archiv, Bd. 65. Schülein: Ibid., Bd. 66. Wegscheider: Ibid., Bd. 68.

¹ Rosenthal: Deutsche med. Wochenschrift, 1888.

² Read before the Philadelphia Academy of Surgery, March 5, 1888.

of a probe-pointed pair of scissors, the tunica vaginalis was then dissected from the surrounding tissues of the scrotum, until its attachment to the cord and testis was reached. The loose tunica vaginalis was then cut off close to the testis and cord, and the wound was accurately closed by five sutures, horse-hair drainage being provided. No hemorrhage of importance occurred from the sac or during its dissection.

Strict antiseptic precautions were used (but no spray), carbolic acid 1 : 24 for the instruments, and bichloride of mercury, 1 : 1000, for the hands, sponges, etc. A sublimate gauze dressing was then applied and retained by a broad T-bandage, the abdomino-scrotal portion being triangular in shape, with an aperture for the penis.

At 7 P. M. of the same day, five hours after the operation, the patient's temperature was normal, his pulse was 58, and he was suffering some pain. The temperature rose on the following evening to 100.2° F., which was the highest point attained, and from which it gradually declined, to reach the normal standard on the fifth day after the operation.

The dressings were changed on the first day after the operation, and were nearly saturated and stiff from the oozing of bloody serum.

On the third day, at the second dressing, three of the five sutures were removed, as was also the horsehair used for drainage.

On the seventh day the two remaining sutures were taken out, the dressing discontinued, and the patient allowed to get up with the scrotum in a sling. Beyond the slight pain on the evening after the operation, the patient suffered no inconvenience, except that incident to confinement to bed. He was entirely well on the 31st, but for other reasons did not actually leave the hospital till January 7th.

REMARKS.—Until recently various methods of injection, especially by iodine or carbolic acid, have been the chief and almost the only means employed in the treatment of hydrocele. They have the great advantage that but few deaths can be laid to the use of iodine, and none, I believe, to carbolic acid. But they have several disadvantages.

First, that of recurrence. In over eight per cent. of the cases treated by iodine, recurrence takes place, and this misfortune sometimes even occurs a second time. Carbolic acid has been much less widely used, but its promise as to freedom from recurrence is much better than iodine, and if injection be the method chosen, this would seem to be the preferable one.

Secondly. In a large number of cases additional lesions are found besides the hydrocele, lesions which in the majority of cases cannot be cured by any injection. In 123 cases reported by Juillard and Senzmer (*THE MEDICAL NEWS*, May 3, 1884) cysts were found in 43, enlargement of the testicle and epididymis in 23, thickening of the tunica vaginalis in 54, false membranes in 26, and free or attached foreign bodies in 3.

Thirdly. There is generally great pain.

Fourthly. Not seldom severe inflammation and swelling, sometimes suppuration, and occasionally even gangrene of the scrotum occurs.

Fifthly. Even if no such untoward results occur there are great induration, weight, and tenderness in the scrotum, such that the patient is usually laid up for three weeks, and sometimes longer.

Three somewhat different methods of open antiseptic incision have been proposed as substitutes for injection, and especially with a view to a more permanent radical cure. Volkmann incises the sac, and after syringing it out with a three per cent. solution of carbolic acid, unites the edges of the tunica vaginalis to the skin, and, after drainage, dresses the wound with an antiseptic compressing gauze and bandage.

Juillard took a step further. After incision of the sac, and removal of all pathological products by the scissors or spoon from the sac, cord, or testicle, he then resects the superfluous tunic, leaving only enough to cover the cord and testis. The edges of the tunica vaginalis are then sutured with catgut and the external wound closed independently, drainage and antiseptic gauze completing the dressing.

Bergmann (reported by Bramann, *Berlin. klin. Wochenschr.*, April 6, 1885, p. 209) went a step further, and proposed the total extirpation of the free part of the tunica vaginalis, only leaving that portion attached to the cord and testis. Of course, any other concomitant lesions are appropriately treated at the same time. Bramann reports twenty cases, with rapid cure in eleven days, without fever or suppuration. Bull has reported two cases (hydrocele of the cord) thus operated on, one of which had been twice injected with carbolic acid, and the other once with iodine. Both rapidly recovered. Southam (*Lancet*, September 10, 1887) reports four cases, one of which had recurred after carbolic acid injection, cured with an equally speedy and happy result.

To these twenty-six cases I add but one, but it is an excellent illustration of the slight reaction following the operation, and of the quickness of the cure.

It would seem, *first*, to be almost an impossibility that recurrence should take place, for practically none of the sac is left, and in so far it would be *a priori* the ideal radical operation.

Secondly. This method facilitates the proper treatment of any additional lesion above described as not uncommon.

Thirdly. As is usual after antiseptic wounds, there was little or no pain.

Fourthly. As again is the rule in wounds so treated, there was little inflammation, practically no fever, not a drop of pus, and no seeming possibility of gangrene.

Fifthly. There were only moderate induration,

weight, and tenderness, so that the patient was up and about on the seventh day, and was ready for discharge on the eleventh day entirely cured and comfortable.

Sixthly. Although a more radical operation than that of Volkmann, it is less painful and less dangerous. In 254 cases collected by the writer in THE MEDICAL NEWS (*supra*) recurrence is noted in three, and two died of septicæmia and pyæmia, while abscess, sloughing, and orchitis have all occurred. Induration, weight, and tenderness have persisted from three to four weeks. The conditions after resection of the entire tunica vaginalis and immediate closure of the wound, with drainage, and antiseptic dressing are such as to favor the speediest and safest recovery, without any of the dangers above noted as following antiseptic incision.

The case is also a good illustration of "the conditions conducive to the *earliest possible* healing of operation wounds," to which I have called attention in a paper with the above title, read before the Surgical Section of the New York Academy of Medicine (*Medical Record*, January 28, 1888), especially the method of drainage by horsehair instead of rubber, glass, or bone tubing, as is suited best to so small a wound, and the change of dressing on the day after the operation, recovery being complete in seven days.

SALICYLATE OF SODA IN POLYURIA.

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ON June 23, 1887, Mrs. L. brought to my office her daughter, aged eleven years. The child was of large frame for her years, but pale and flabby, and though the weather was warm she complained much of cold hands and feet. The mother informed me that the child was voiding too much urine. They were requested to return after a lapse of twenty-four hours, during which the mother was to determine accurately the measure of the urine. This was found to be nine and a half pints (milk measure). Urine was found to contain no sugar. Fluid extract of valerian in half drachm doses three times daily was given for ten days to no purpose. Thirst during this time was insatiable. The child was directed at the first visit to quit her attempts to learn music, to do as little work or play as possible.

Ergot and tannic acid were given next for ten days; the former in half drachm doses three times daily, and five grains of the latter, to be given alternately. They did no good.

It was now near the middle of July; the thirst was difficult to appease, the quantity of urine was as great as before, and the child was weaker and further reduced in weight to seventy-nine pounds. The urine was found, as before, to be free from glucose.

She was now given eight grains of salicylate of soda in aqueous solution after each meal; her meals were small. In ten days there was an appreciable

amendment; she had more solicitude about her food; she felt stronger. The treatment from this time forward, consisted of nothing else than the salicylate of soda; the dose of eight grains was increased to ten, but so much complaint of headache was excited that I was obliged to suspend dosing for a few days. No restriction was imposed upon the choice of food.

The amount of urine diminished slowly but steadily until, in November, the daily discharge was two and a half pints. In the meantime her color returned, there was no longer complaint of lassitude and of inability to breathe easily. Her weight was increased to eighty-seven pounds. She was sent to school, and has manifested no illness of any sort since. The remedy was discontinued early in December.

I am aware that little importance is to be attached to a single result of this sort, and regret that further opportunity has not been mine to test the efficacy of this remedy; but cases of polydipsia are not of common occurrence, and, rather than wait until time shall bring to me chances to test the value of this remedy, I submit this report for what it is worth. I regret that I am unable to state the duration of this affection prior to my first acquaintance with the case. The mother was aware that the child had been obliged for weeks to rise repeatedly during the night to void her urine, but was astonished at its volume when she measured it. It is scarcely necessary to add that the mother's attention had been excited by the consuming thirst for weeks before she took any measures for its relief, or before she deemed any explanation necessary.

NOTES ON THE PAST AND PRESENT MORTALITY AND TREATMENT OF PNEUMONIA.¹

By HENRY HARTSHORNE, M.D.,
OF PHILADELPHIA.

OBSERVATION and experience, beginning more than forty years ago, along with such attention as has been within my power to current medical literature, have made me acquainted with a process of transition going on in medical practice, especially in regard to the treatment of acute inflammatory affections, of which pneumonia may be considered as a type. Between 1845 and 1855, the accepted treatment of pneumonia was what was then called moderately antiphlogistic; including early and moderate abstraction of blood in patients of good strength and not over middle age, early purgation with some active cathartic medicine, and then the use of agents, chiefly mineral salts, to promote and maintain the action of the skin, kidneys, and bowels; very little use of opium being made, unless at a late stage, and quinine being reserved for that period as a tonic; alcoholic stimulation being resorted to only in cases of exceptional prostration, as in aged patients, or in

¹ Abstract of a paper read before the College of Physicians of Philadelphia, February 1, 1888.

those of enfeebled constitution, or when a severe and prolonged attack brought on decided exhaustion.

Under this treatment, in private practice, patients with uncomplicated pneumonia and pleuro-pneumonia very generally recovered; and in hospitals their mortality was hardly more than one death in ten cases; often considerably less than in that proportion. In Suffolk Hospital, England, in 1835, it was only one death in fifty cases.

Between 1855 and 1860 a movement of reaction went on against bloodletting, in which Dr. J. Hughes Bennett was one of the most conspicuous leaders. This was shortly followed, in part, indeed, accompanied, by the introduction, under the leadership of Dr. R. Bentley Todd, of the early and free use of alcohol in pneumonia and other acute inflammatory diseases. Next came the era which still continues, of physiological rationalism in therapeutics; characterized by a general abandonment of previously approved principles and methods, and the substitution for them of the use of potent agents upon special indications in regard to functional actions and conditions, these agents being first tried in the laboratory upon animals, either in a state of health or in various conditions of traumatism. Prominent among the agencies thus lately much used in practice are those designed, as "apyretics," directly to lower the temperature of the body when it is abnormally elevated in disease.

The "working theories" of practice in acute inflammatory diseases have thus, with very little appearance of distinct formulation, undergone a gradual, but very positive change. To-day the predominant method of treatment of pneumonia may be said to be characterized by the following features: 1, the practically universal omission of venesection, and the very rare local abstraction of blood; 2, the general disuse of active cathartic medicines in the early stage; 3, by many, perhaps a majority of physicians, the early and continued use of alcohol, to the extent of from 2 to 12 or more fluidounces in 24 hours; quinine, mostly in 10 or 20 grain doses, once or twice daily; opium, or morphia, from the start or near it, averaging perhaps sulphate of morphia gr. $\frac{1}{2}$ to gr. $\frac{1}{4}$, every 3 or 4 hours; with deviations from this general plan, or additions to it, by the use of antipyrin, antifebrin, aconite, digitalis, etc.; and warm applications, as poultices, or cotton batting to the chest, or counter-irritation with turpentine, etc. These last may be said to be common to the old and the new methods of treatment.

For definite information sustaining this account of now predominant practice, I refer to Hospital Notes, published last year, of hospitals in New York, Boston, and Philadelphia; also, memoranda furnished me during last year by a resident physician in one of our largest and best hospitals; and the *dicta* concerning treatment, of Dr. A. L. Loomis, in his

article on "Croupous Pneumonia," in vol. iii. of the *American System of Practical Medicine*.

It is entirely legitimate to apply to these old and new, different, and in some respects opposed, methods of practice, careful reasoning as to the principles upon which they are based. Considerable attention is given to this kind of comparison in the paper of which this is an abstract. Yet it is also appropriate, and is more nearly decisive in importance, to bring to bear upon them the testimony of *facts* concerning the *results* of the different modes of treatment which are compared. It is true that an absolute demonstration of therapeutical conclusions by means of statistics is not often practicable. My opinions on this subject have not been founded upon statistics, but upon direct personal experience. But, when evidence of that kind, of a marked character and considerable in amount, can be adduced, it is manifestly worthy of careful attention, even when the conclusions to which it points are different from those which, in practice if not in theory, are in vogue, and which are approved by highly respected authorities, at the present time. Such evidence it is the principal purpose of my paper to bring forward, and to apply to the question, how we ought to treat acute, uncomplicated pneumonia, in patients of ordinarily good constitution, not over middle age.

From sources and authorities which will be admitted to be reliable, and which are referred to in full in the paper of which this is an abstract, I have obtained and analyzed statistics, of which I will now give a summary account. I will say that, throughout my analysis of these statistics, I have made liberal allowance in favor of that construction of the facts which is most opposed to the conclusion which my judgment approves. On behalf of the latter, a much stronger case might be made out, if my aim were anything other than the actual truth.

First, a comparison is made of the results of different modes of treatment of pneumonia, within a few years prior to 1858, reported by between forty and fifty physicians, of 11,627 cases in Great Britain and Ireland and on the continent of Europe. Of these, 2751 cases were treated with bleeding, and 8876 without it. Among the latter, however, 452 cases had exceptional modes of medication in the use of chloroform, lead, copper, or iron. Deducting these, the comparison rests between 2751 cases treated with, and 8424 cases without bleeding. The figures, in summary, are these:

With bleeding, either often, a few times, or once, with or without tartar emetic, altogether, 1 death in 11.1 cases.

Without bleeding, under all treatments except with opium—except, also, with chloroform, copper, or iron, as before said—1 death in 9.7 cases.

Large and repeated bleedings, alone, 1 death in 11.6 cases.

Bleeding a few times, or moderately, alone, 1 death in 12.3 cases.

Bleeding and tartar emetic, 1 death in 12.56 cases.

Tartar emetic, no bleeding, no opium, 1 death in 11.3 cases.

Opium, without bleeding, 1 death in 3.3 cases.

Tartar emetic and opium, without bleeding, 1 death in 3.8 cases. I would call attention especially to these last figures, for the greatest mortality obtained under any method was that under the opium treatment of pneumonia.

Next, I present a comparison of mortality from pneumonia in the United States Army, at two periods: one before the anti-bloodletting movement had fairly set in, and the other, when it had begun to make a decided impression upon general practice. Between 1840 and 1854 there were, in the United States Army, 1416 cases of pneumonia, with 127 deaths—1 in 11.15; between 1855 and 1859, 657 cases, with 97 deaths—1 in 6.67; an increase of more than one-third in the proportion of deaths.

The testimony of Dr. George B. Wood, in the first edition of his *Practice*, published in 1847, was decided as to the favorable prognosis of uncomplicated pneumonia, especially in early life. Dr. Lewis P. Gebhard, of Philadelphia, whose practice began near the beginning of this century, and was extensive for many years, told me, near the end of his life, that he had never lost a case of simple pneumonia.

Examining the records of the Pennsylvania Hospital, as a representative institution, with a medical staff excelled by none in reputation and ability, I have found the results to be as follows: in the three years, 1845, '46, and '47, from pneumonia and pleuro-pneumonia, 1 death in 16 cases—6¼ per cent.; in the years 1865, '66, and '67, 1 in 51.6th, or 18½ per cent.; in 1884, '85, and '86, 1 in 3.2, or more than 31 per cent. To make sure the avoidance of the error of confounding cases of acute tuberculosis with pneumonia, in this comparison, I invariably took only those fatal cases in which the diagnosis of pneumonia was entered at the time of the death of the patient. In collating some other statistics of the same hospital, which were cited in the discussion upon my paper in the Philadelphia College of Physicians, I believe this precaution was not taken; and hence a greater mortality was inferred, throughout the time investigated.

Dr. A. L. Loomis, in his article on "Croupous Pneumonia" in the *American System of Practical Medicine*, states that the average ratio of deaths from pneumonia to those from all diseases together, in New York, was 15.2 per cent. greater between 1859 and 1877 than between 1840 and 1858.

Other figures are given more fully, with references to their authorities, in my paper. The figures may be altogether summarized thus; first, all prior to, or not later than, 1858:

Skoda estimated the average mortality of pneumonia, about 1841, as 1 death in 8 cases. Balfour, near the same time, reported his observation in a homœopathic hospital under Fleischmann (treatment probably *nil*), of a mortality of 1 death in 6½ cases; Dietl, with diet only, no bleeding nor medication, 1 death in 13.5 cases; in the British army, at home and in various stations, average of all together, 1 death in 20.66 cases; hospitals in a number of different cities in Europe, from 1822 to 1856, average 1 in 9.54; United States Army, as already said, from 1840 to 1854, 1 in 11.15; Pennsylvania Hospital, as before mentioned, 1845, '46, '47, 1 in 16. Taking all these together, it is safe and fair to estimate the average mortality of pneumonia, during the second quarter of this century, as not more than 1 death in 12 cases, or 8.33 per cent.

Coming now to the recent and present mortality of the same disease, we find it editorially stated in *THE MEDICAL NEWS* of December 11, 1886, that "the rate of mortality" of pneumonia "in the large general hospitals in this country is rarely below, more often above, 25 per cent., which represents about the average death-rate from this disease in the Northern and Southern armies during the Civil War." It is added, that "in this country extensive statistics of pneumonia in private practice are not available; but in the recent returns of the Collective Investigation Committee of the British Medical Association, the mortality was 18 per cent." This last ratio, it will be perceived, is considerably more than double the carefully computed mortality of pneumonia before 1858, namely, 8.33 per cent. We have thus, I think, a *demonstration* of the large increase in the proportion of deaths from that disease in recent times, over what it was thirty, forty, and fifty years ago.

How is this increase of mortality to be accounted for? Three hypotheses are conceivable.

1. The records may be supposed to have undergone modification through improved methods of diagnosis. There is no ground for this supposition. The physical signs, as well as the symptomatology, of pneumonia, were well understood forty and more years ago, by those physicians whose reports have furnished materials for the statistics which have been cited. The use of percussion, in aid of diagnosis, dates with Auenbrugger, 1761; auscultation began with Laennec, about 1818.

2. "Change of type" of the disease may be asserted; or a general change in the constitutions of men, affecting the character of all diseases. Reasons are given in my paper for believing that neither of these hypothetical assumptions has such support in facts as at all to meet the requirements of the case. While, at all periods, it may be sometimes sthenic and sometimes asthenic or typhoid, pneumonia is practically the same disease that it was forty or fifty years ago.

Lastly, therefore, we are brought to dwell upon the coincidence between this great increase in the mortality of pneumonia and a marked change in prevailing methods of treatment.

What has brought this change of treatment about? Is it a better knowledge of the pathology of pneumonia? No. Autopsic and microscopic investigations have cleared up certain minutæ, as to the distinction, for example, between croupous and catarrhal pneumonia. But we still have, as ever, the first stage, of congestion; and, the second, of red hepatization of the lung; in other words, primary *stasis*, with interrupted nutrition, at the focus of inflammation; concentric hyperæmia there, and general vascular excitement of the system; then exudation, whose changes chiefly determine the result, as least so far as the lung is concerned. The increased vascular tension following a local obstruction to the circulation, gave the indication for measures of relief in the older practice; and that indication still remains, in the presence of the most advanced researches as to either the crass or the minute morbid anatomy of pneumonia.

Attention is given in my paper, at length, to the opinion now entertained by some pathologists, that pneumonia is a systemic fever, with lung changes as merely its local manifestations; especially in connection with the theory of the microbic causation of the disorder. Jaccoud, in France, and Dr. H. B. Baker, in Michigan, are cited as having not long since¹ given *demonstrative* evidence that the essential, necessary cause of pneumonia *cannot* be microbic; dry cold having the most to do with its production, in at least much the larger number of cases; while the bacilli and micrococci often found present may, no doubt, exercise a morbid action, affecting the character of the disease. If, however, it were even proven that pneumonia depends upon microbes for its causation, that would not decide the question of its treatment, while we are not in possession of direct and certain microbicidal measures of therapeutics.

By all these considerations we are led at last to the conclusion, that the change in practice that has been referred to, has no other origin than a fluctuation of opinion, not justified by facts or reasoning, concerning certain principles in therapeutics. This fluctuation involves:

1. A depreciation of the value of the early abstraction of a moderate amount of blood, locally or by venesection, for the relief of active inflammation, in persons of good previous health, and not advanced in age.

2. A like depreciation of the utility of early

¹ Jaccoud's paper was communicated to the Académie des Sciences, and was reported May 7, 1887, in *La France Médicale*. Dr. Baker's investigations have been made public in several places within a year or two.

catharsis, especially as an eliminative agency, to prevent the accumulation in the system of the products of regressive metamorphosis of the tissues.

3. The same want exists of due valuation of salines, formerly called refrigerant medicines, for lowering blood-pressure and promoting the functional activity of the skin and kidneys, as well as the bowels.

4. An erroneous, exaggerated estimate prevails of the beneficial influence of large doses of quinine, employed for the reduction of temperature. Testimony is accumulating as to the failure of the use of quinine in antipyretic doses in the treatment of pneumonia. Drs. Bartholow and Osler, in Philadelphia; Drs. Kinnicutt, Ripley, Putnam Jacobi, Castle, Billington, and Emmett Holt, in New York; and Drs. Shattuck and Minot, of Boston, are among those who dissent from the now common approval of this point of practice.

5. An especially injurious error consists in the administration of opium and morphia at an early stage of pulmonary inflammation. The tendency of opiates to *diminish bronchial secretion* stands right in the way of their utility in pneumonia. I may here recall the statistical fact, mentioned a little while ago, that in the reports of results of different modes of treatment before 1858, the worst record of all was attached to the opium treatment of pneumonia.

6. An experimental administration is now being carried on of antipyrin, antifebrin, etc., as antipyretics, whose results do not correspond with what is desired of them as remedies for acute organic inflammation. They have another rôle, as neurotic medicines, in which they possess much value. I may remark incidentally, as an example, that in Dr. Pepper's interesting case, reported in the *Medical and Surgical Reporter* early in last year, it appears to me probable that it was the *neurotic* element in the case—the strongly marked tendency to convulsions—that received benefit from the antipyrin used rather than the pneumonia itself.

7. The practical ignoring must be noticed, in recent therapeutics, of the important difference between the debility of *oppression* in the early period of severe attacks of acute disease, and the *exhaustion* which belongs to a later stage of the same, or which is produced by other causes and conditions. The *treatment* of these two states, as was long ago recognized, is properly quite different.

8. Connected with this, the early and free use of alcohol is now common. In my judgment, except in previously enfeebled patients or those otherwise exceptionally prostrate, this is an injurious medication in pneumonia. Heart-failure during the early stage, in patients of previously good health, may be best averted by so lightening the burden of oppression imposed upon the heart, as to make it possible for it to carry its load without exhaustion. It is under a prevailing so-called "supporting" treatment that

not a few men in the prime of life have, within a few years, dropped off under less than a week's illness with pneumonia.

The relief often afforded by bleeding, without consequent exhaustion, even at a somewhat late stage, to the condition characterized by distention of the right heart (this being a practice approved even by many physicians who are otherwise opposed to bloodletting) should be remembered and set over against the excessive fear entertained by some of the production of debility by early bleeding.

I conclude, then, that there is reason for the judgment that the now current "working theory" of the treatment of pneumonia and allied affections, by early and continued stimulation and narcotism, is not supported by the facts concerning the results of that treatment, as compared with those of the moderate, early, sedative and eliminative practice of forty and fifty years ago.

I do not assert that venesection is called for in nearly all cases; but I believe that early local depletion will do good in the majority, in patients not old, and who were previously in good health; and that early active catharsis, with diaphoretics and diuretics afterward, are rightly in place: quinine as a tonic, and alcohol as a stimulant, being indicated only exceptionally at the beginning, in persons of low vitality or bad habits, or old age; more often, when real exhaustion is imminent, at a late stage.

There is more room for question, on the basis of experience, between the older practice and pure expectancy, nursing the patient in bed without medication, than there is between the therapeutic methods now current, in regard to acute inflammatory affections, and those of the early part of this century. We want no return to any Sangrado extreme; but only to replace in their position of confidence, lost for a time, some of those measures of practice which have been abundantly tested and commended by the experience of ages.

MEDICAL PROGRESS.

The Pulse in Children.—JACOBI, in the *Brooklyn Medical Journal* for March, 1888, writes as follows on this point:

The pulse of the infant and child offers a great many differences from that of the adult, both in frequency and volume. As already stated, the pulse of the fœtus varies from 124 to 144 or 150 or more. Immediately after birth it is very much less frequent. Within an hour it assumes a certain regularity, and still the figures furnished by a large number of competent authors appear to prove the greatest difficulties in obtaining uniform results. From what I have observed, J. L. Smith is correct in fixing the number of heart-beats in a minute at 126, his lowest figure being 96, his highest 164. According to some, the average figure rises during the second half of the first month, and then proceeds slowly to decrease. From the first to the sixth month the pulse is 120 during sleep, 130 to 135

while awake. It is 100 at six years, 88 at thirteen, 72 in the adult. Tall children exhibit less frequency than short ones; girls, after the fifth year, and more so about puberty, more than boys. The pulse is more rapid in the infant while awake, sitting up, or standing, than while asleep or lying down; more in excitement, exercise, or fever. Thus it is difficult to arrive at a safe estimation of the frequency of the pulse in cases of sickness. In the radial artery it is sometimes impossible to obtain it; the femoral or carotid is often more accessible, the basilar, through the open fontanelle, very much more so. When frequency alone is the object of examination, it is always better not to touch the baby at all. The beats of the fontanelle or the carotid can be distinguished and counted easily, up to a frequency of 240 a minute. But it is not always, or not only the frequency we wish to notice, but the character of the pulse. In the latter respect the young have their peculiarities. The expansion of the artery is very much more perceptible and prolonged than the contraction, and the rhythm is by no means stationary. The pulse is very apt to be irregular, even during the regular respiration of sleep. The slightest deviation from the normal standard of health renders it slightly but perceptibly irregular; anæmia does the same, and either increases or diminishes its frequency. When the latter takes place, the differential diagnosis between anæmia and incipient meningitis, with its pneumogastric irritation, becomes quite difficult.

Quinine Hypodermatically.—DE BRURMANN and VILLEJEAN, in an extended article in the *Bulletin Général de Thérapeutique* of March 15, 1888, report the following formulæ for solutions suitable for the hypodermatic exhibition of quinine.

An aqueous solution:

| | | |
|---------------------|-------|---------|
| Quinæ hydrochlorat. | . . . | gr. 15. |
| Aquæ destillat. | . . . | 3 3/4. |
| Aquæ lauro-cerasi | . . . | 3 i. |

Dissolve out at gentle heat and filter.

An alcoholic solution:

| | | |
|---------------------|-------|---------|
| Quinæ hydrochlorat. | . . . | gr. 15. |
| Alcohol (60°) | . . . | ℥ 45. |
| Aquæ destill. | . . . | ℥ 90. |

A solution in glycerine:

| | | |
|---------------------|-------|---------|
| Quinæ hydrochlorat. | . . . | gr. 30. |
| Aquæ destillat. | . . . | 3 i. |
| Glycerin. | . . . | 3 i. |

Dissolve perfectly by warmth.

The Effect of Antipyrin upon the Kidneys.—The *Medical Press* states that at the Société de Thérapeutique M. HUCHARD recently resumed the discussion on certain indications and counter-indications of antipyrin. If the drug gives incontestable satisfaction in many cases, there are others where it should be employed with precaution, and even rejected entirely. In facial neuralgia, migraine, sciatica, etc., antipyrin is generally very valuable, but it has been proved by recent researches that the quantity of urine is notably diminished under its administration, consequently its counter-indication is evident in certain maladies of the kidneys, and in those affections in which some renal mischief is observed as a concomitant symptom. Again, if in the neuralgic form of

angina pectoris observed in nervous and hysterical persons antipyrin eases the pain, it would be dangerous, on the contrary, to give it in true angina with stenosis of the coronary arteries, collapse of the heart might be the result. However, there is one affection which seems to be greatly benefited by the treatment with antipyrin, and that is polyuria. He had actually under observation a woman who was suffering from meningo-myelitis consecutive to an attack of typhoid fever. This woman drank very large quantities of liquid, so that the urine passed in twenty-four hours exceeded twenty quarts! Antipyrin was given in progressive doses up to two drachms in the twenty-four hours, and the result was a rapid decrease in the amount of urine, until three quarts daily were reached. If he had been so fortunate in this case, M. Huchard said that he had no doubt that his success depended on the fact that his patient was of a very nervous and excitable temperament, with exaggerated reflexes and shooting pains down the vertebral column and along the limbs. The kidneys were in no wise affected.

Dermoid Cyst of the Mediastinum.—At a recent meeting of the Berlin Medical Society, DR. LOEWENMEYER brought forward a tumor of this kind, rather larger than a child's head. It was removed from the body of a man who had been shown to the Society four years previously, his symptoms being then difficult to interpret. Apart from hæmoptysis and a moderate degree of pleuritic effusion on the left side, the attention was chiefly drawn at that time to a projection of the left side of the thorax, with loss of resonance, reaching from the clavicle to the sixth rib. The apex beat could not be felt over the cardiac region, but a thrill was noticed to the right of the sternum, also epigastric pulsation. A precise diagnosis was not made. The patient improved somewhat in hospital, and then went out and worked for a few years regularly, but no symptoms of pressure came on till quite recently, when severe recurring attacks of dyspnoea and cyanosis quickly induced a fatal result. The necropsy revealed a tumor occupying the greater part of the left half of the thorax. Anteriorly it was in contact with the chest wall, having pushed the heart entirely over to the right. Prof. Virchow had examined the tumor, which contained in its interior cysts lined with epithelium and filled with soft substance of gelatinous consistency, but the outer and firmer portion of the tumor was composed of dermoid products, namely, epidermis, hair, fat. Cartilaginous plates were also found. Evidently there had existed abnormality of the chest wall in the embryonal stage of existence of the patient, so that part of the integument had been displaced inward, and part of the respiratory apparatus outward, at the time of closure of the thorax.—*British Medical Journal*, March 17, 1888.

Diabetic Foods.—HARRINGTON, of the Harvard Medical School, publishes in the *Boston Medical and Surgical Journal* of March 22, 1888, the following conclusions, based on an examination of the diabetic foods most commonly sold:

Comparing the estimated figures with the amounts of starch in the two ordinary breads, it is observed that the difference is but slightly in favor of the diabetic breads, whereas, according to the claims made by the manufacturers and retailers, the difference should be overwhelming. But the item of palatability is something to

be considered, and the slight difference in the percentage of starch might well be waived in favor of the great difference in taste.

There is another and more serious side to this question of diabetic foods. That they are in the highest degree fraudulent has, I consider, been proved. They are in addition a positive danger, for the diabetic accepting as truth the assertions that they are non-starchy, takes into his system that which even the circulars of the manufacturers admit to be virtually a poison in its effect on the course of the disease, and thus innocently more than counteracts the benefit which he otherwise would derive from his medical adviser. In conclusion, I have to express the hope that every practitioner will do his utmost to discourage the use of these fraudulent and dangerous foods, and to drive them out of the market.

A Pill for Incontinence of Urine.—GRISOLLE prescribes the following:

| | | |
|--------------------|---------|----------|
| R.—Ext. nucis vom. | | gt. 3. |
| Ferri oxid. | | gr. 45. |
| Quassia pulv. | | gt. 45. |
| Syrup. simpl. | | q. s.—M. |

Ft. pil. 20 in num.

Sig. 1 to 3 pills daily.—*Les Nouveaux Remèdes*, March 8, 1888.

The Treatment of Fissures of the Nipple.—MONTI advises the use of gutta-percha, chloroform, q. s., to dissolve. Excoriations should be covered by a pellicle of the dissolved gutta-percha, which is not removed by nursing.—*Les Nouveaux Remèdes*, No. 4, 1888.

The Practical Use of Haya to Produce Anæsthesia.—The *Medical Press* of March 14, 1888, writes that at a meeting of the Berlin Medical Society KAREWSKI gave an address on the subject. The preparation employed was obtained from Merk, of Darmstadt. Complete anæsthesia was never obtained, but its action was much heightened by the local production of anæmia. Its action was not uniform in all cases. Subcutaneous injection of at least $\frac{1}{8}$ of a grain was necessary to produce analgesia. The analgesic effect of plugging the nares with 0.5 per cent. solution took place after eighteen minutes. It did not act over an area of more than a half an inch above and below the point of injection. The analgesia thus set up sometimes lasted several hours, the length of time being dependent on the size of the dose. The bye effects were very disagreeable, amongst them being violent pain at the point of insertion, coming on in a few minutes afterward, and becoming intolerable and lasting for several days. The results were not encouraging, but in a number of cases of neuralgia some favorable ones were obtained. Two cases of lumbago, one of brachial neuralgia, one of intercostal neuralgia, and one of sciatica, were treated by parenchymatous injections varying from $\frac{1}{10}$ to $\frac{1}{4}$ of a grain. Great pain was produced which lasted for an hour and a half, after which it subsided, and with it the original pain, the latter remaining away twenty-four or more hours, and in some cases permanently.

The Initial Angina of Typhoid Fever.—CADET DE GASSI-COURT, in *L'Union Médicale* of March 15, 1888, reports three cases of angina, in whom the quinsy was the first

symptom of typhoid fever. Two of the cases were in children, both were of moderate severity, and terminated in moderately severe typhoid fever; both patients recovered. The third case was a boy, twelve or thirteen years of age, vigorous, and, when first seen, in violent delirium. The pulse was rapid; the skin hot; the sub-maxillary glands swollen, and the tonsils and soft palate covered with a whitish yellow exudate. While the diagnosis of diphtheria was doubtful, symptoms of typhoid developed, and the patient died on the twenty-fifth day.

The difficulties of diagnosis, especially when the history is unattainable, are very great.

Displacement of the Testicle.—At a recent meeting of the St. Petersburg Society of Russian Practitioners, PROF. VLADIMIR N. POPOFF showed (*Vratch*, No. 4, 1888, p. 75) a unique case of ectopia of the testicle in a patient aged twenty-four. The scrotum was found to be normally developed, with the right testicle in its usual situation; but the left half of the scrotum was empty, while exactly at the root of the penis there was a swelling of the size of a walnut, covered with normal skin, and containing an oval body which measured about four-fifths of the normal testicle, but felt somewhat softer. The patient said the swelling had been there since birth. He had, however, suffered in early childhood from an inguinal hernia, and Prof. Popoff thinks it more likely that the displacement of the testicle developed under the influence of the rupture. The man's sexual power was formerly quite normal, but for about six months he had been impotent. Dr. Popoff could find no similar case in medical literature. Prof. Wenzeslaw L. Gruber, the great Russian anatomist, who takes special interest in anomalies, considered the case unique.—*British Medical Journal*, March 17, 1888.

Ergotin Hypodermatically, for Hemorrhage.—MONTARD-MARTIN prescribes the following solution:

| | |
|-------------------|---------|
| Ergotin | gr. 30. |
| Glycerin. | 3 3/4. |
| Aquæ | 3 3/4. |

Of this solution from 15 to 25 minims may be injected.
—*Les Nouveaux Remèdes*, March 8, 1888.

When should Antipyrin be Given?—The *Medical Press*, of March 14, 1888, furnishes timely information on this point, as follows:

Antipyrin should be administered with or immediately after a meal, otherwise pain, nausea, and discomfort, may result from its contact with the walls of the stomach. The cutaneous manifestations which sometimes follow its ingestion are probably due to vaso-motor disturbances characterized by peripheral dilatation of the arterioles. In certain cases the rash has simulated that of scarlatina, with intense itching. There is every reason to suspect that in consequence of the present great demand for the drug, due care may not be employed in its manufacture, and it is suggested that an analysis should be ordered whenever toxic symptoms are observed. DR. DUJARDIN-BEAUMETZ claims to have detected a mixture of benzene in certain samples, the presence of which would explain many untoward effects. It should be borne in mind, especially by the public, that we have in antipyrin a useful but potent agent, the use of which in unskilled hands may and will in a certain proportion of cases give rise to severe

and even fatal symptoms. Manufacturers would also do well to look a little more closely to the purity of the drug, otherwise it may fall into discredit.

Calycanthus glaucus (Carolina Allspice).—ECCLES writes, in the *Brooklyn Medical Journal* for March, 1888, that the shrub within whose seeds he discovered the alkaloid, was during the late civil war used in decoction of roots, leaves, and bark by the Confederate soldiers for the cure of intermittent fever, and, as claimed, with success. It is still used in domestic practice by the natives of the region where it grows. A fluid extract of an allied species is already upon the market, so that somewhere in the country it is being prescribed for some purpose. If in its crude form it has proven of advantage, this new concentrated form should be still more efficient. The seeds contain nearly two per cent. of this alkaloid, and a smaller amount of probably two others. The odor of the volatile one of these last is distinctly that of pyridine, an alkaloid of tobacco, and as it is unlikely that two should exist having the same smell, we may at present assume it as probably such.

Antifebrin in Epilepsy.—BOROSNYOI is quoted by the *Centralblatt für die gesammte Therapie* for March, 1888, in his report of the results obtained by using antifebrin in the treatment of epilepsy. He treated nine epileptics with the remedy in doses ranging from three and a half to thirty grains. It was found necessary to intermit the treatment by pauses. His conclusions are that antifebrin is inferior to the bromides in the treatment of epilepsy; cases of collapse and cyanosis were observed during the treatment. Large doses of antifebrin control epileptic paroxysms, but are too depressing to be continued.

Sulphurous Inhalations for Phthisis.—A series of observations has recently been made on a method of treating pulmonary tuberculosis, which, if not curative, would appear to possess a beneficial power over the progress of this dread malady. The method consists in the systematic inhalation of an atmosphere impregnated with the fumes of sulphurous anhydride (SO₂); and several ingenious plans of obtaining a constant supply of the gas have been devised, the best of which is a lamp constructed to burn bisulphide of carbon. The simple plan of burning flowers of sulphur in a closed room can, however, be resorted to if desired. Under its influence, it is said, the expectoration becomes more liquid, the mucous surfaces are relieved from the irritation caused by the presence of muco-purulent secretions, and the patient is spared the fatigue of violent paroxysms of cough. In a certain number of favorable cases the improvement in the general health which follows, is reported to be sufficient to allow of the cicatrization of cavities, and the subsidence of the more disquieting symptoms. The irritating effects of the vapor may be mitigated to some extent by burning opium and gum benzoin at the same time. Some two hundred observations have already been recorded, and the results have been sufficiently good to warrant a more general trial. Caution is advisable in the quantity of the gas, which should not exceed a certain proportion, which must be ascertained by close supervision of the patient during its administration.—*British Medical Journal*, March 17, 1888.

THE MEDICAL NEWS.

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SATURDAY, APRIL 7, 1888.

THE RELATION OF THE ACIDITY OF THE GASTRIC JUICE TO THE DIAGNOSIS AND TREATMENT OF DISORDERS OF THE STOMACH.

DIFFERENT opinions concerning the nature of the acid to which the gastric juice owes its reaction have prevailed since the days of Blondlot and Bernard. Various regarded at different periods by men of the highest attainments in chemistry and physiology, as phosphoric, sarcolactic, lactic, and hydrochloric, the discussion is now limited to the two last named substances; and, with reference to these, the latest researches have proved that either may be present alone, or both in combination. During the first half hour of normal digestion, lactic acid is found alone; then follows a stage in which it is in company with hydrochloric; finally comes a period—the longest and most active—in which the latter is the only acid present.

To determine the digestive power of the stomach, its secretion should not be removed for examination until at least three-quarters of an hour after the ingestion of food—that is, during what has been called the hydrochloric acid stage. The object of the examination is to determine, first, the presence or absence of hydrochloric acid; secondly, its amount. The tests employed for this purpose are too numerous to describe at length here. As in examining urine for sugar or albumin, so here each observer comes to rely upon that method with which he has had the greatest experience. The latest test for hydrochloric acid in the gastric secretion is the

phloroglucin-vanillin test devised by Günzburg, of Frankfort, of which a full description was given in THE NEWS of January 14, 1888. Concerning this test, Ewald declares that it is the best and most exact of any yet employed, and there is little doubt that with this, and the well-known methyl-violet test, the investigator will be fully equipped.

The practical results that have thus far been derived from these clinical applications of chemistry are interesting and valuable. The first examinations of the contents of the stomach led to the belief that hydrochloric acid was invariably absent in cancer of that organ, and this supposed fact was eagerly welcomed as a sure means of determining or excluding carcinoma. From a careful study of five cases, in which he made 154 examinations of the gastric juice, Riegel concluded that, as a rule, the gastric juice of a cancerous stomach contains no hydrochloric acid, and is devoid of digestive power, and that, on the other hand, when these are present in a specimen, cancer may be excluded in spite of the most positive symptoms of that disease. The case, however, is not so simple, for free hydrochloric acid is not only absent in other non-cancerous forms of dyspepsia, but has been found in several undoubted cases of carcinoma, in one of which (referred to by Petit, *Gazette Hebdomadaire*, February 10, 17, and 24, 1888) it persisted until death. Seé, who has recently studied this question with the aid of Günzburg's test, concludes that the diseases which reduce the hydrochloric acid to a minimum are cancer of the stomach, with or without dilatation; atrophy, and amyloid degeneration of the mucosa; and certain forms of dyspepsia known as mucous, which result from inanition and marasmus. These are, unfortunately, just the conditions that are most apt to simulate cancer, so that the conviction is forced upon us that too great a significance has been attached to the absence of hydrochloric acid in carcinoma of the stomach. It is still, however, justly regarded as a valuable corroborative symptom in latent forms of that disease.

The opposite condition of hypersecretion is a well-recognized morbid entity, and has been already discussed at some length in THE MEDICAL NEWS of October 1, 1887. Physiologists have established the fact that pepsin displays its greatest digestive power in gastric juice containing a certain proportion of hydrochloric acid, about 2 per 1000. When the acid exceeds or falls below this amount, the digestive power of the secretion is impaired, and, apparently,

in equal degree: that is to say, in a neutral, as well as in an excessively acid secretion, digestive power is entirely absent.

Hyperacidity has been frequently observed in pregnant women with repeated vomiting, in diabetes, in certain cases of dilatation of the stomach, and in chlorotic girls. Its presence in the latter class is quite significant, for hyperacidity is known to predispose to gastric ulcer, which had been observed to be common in cases of chlorosis before the state of the gastric secretion in that disease had been investigated.

The practical question with reference to the chemical examination of the stomach contents is whether it has added to our therapeutic resources. This question may be positively answered in the affirmative. Acids and alkalies formerly administered almost at random, may now be prescribed with mathematical certainty. It has been proved by these researches, for example, that dilute hydrochloric acid which has been fitfully prescribed in typhoid fever, meets a real indication; for in febrile conditions this acid is much diminished. In cancer of the stomach the indications for its administration would seem to be equally prominent. Opinions, however, are divided as to its efficacy. Sée considers it absolutely useless in this disease, since the gastric juice is devoid of pepsin as well as of acid. On the other hand, Petit mentions having seen a case of gastric cancer in which all the digestive troubles disappeared after the administration of hydrochloric acid, with consequent improvement in the general nutrition.

The condition of hyperacidity furnishes more decided indications for treatment. The condition once recognized, the exhibition of alkalies follows, as a matter of course, but does not, as a matter of course, succeed. Failure often depends upon the administration of too small a dose. If not, it should be generally known that small doses of an alkali will actually increase the acidity of the gastric juice, and will, therefore, in cases of hyperacidity act the part of fuel to a fire. On the other hand, large doses not only always relieve the immediate symptoms, but will often exert a permanent curative effect.

CONTRAINDICATIONS AND DANGERS OF ANTIPYRIN.

Of the numerous recent additions to our materia medica, antipyrin appears to have come into most general use, and to have a very wide range of use-

fulness. But its administration appears, upon further study, to be not without contraindications and dangers. ELOY, in the *Revue Générale de Clinique et de Thérapeutique* of March 1, 1888, points out that a large number of ailments are frequently produced by it, particularly if given for any length of time.

He quotes Guttman, Falkenheim, Alexander, and others, in support of his statement that nausea, vomiting, and gastro-intestinal disorders may result from its use. Beside these symptoms, he records instances where syncope has occurred under its employment, and Bentzeff is quoted as believing that the drug always produces such a tendency. He asserts that, in the opinion of Moncovor, Dujardin-Beaumetz, and several others, the secretion of urine is seriously modified, and one of his references is stated to have put it that "antipyrin closes or shuts up the kidneys."

Going still further, Eloy calls attention to the case of Dr. Barr, which is recorded in the *Lancet* of February 25, 1885, in which collapse and death followed the ingestion of from fifteen to thirty grains of antipyrin in two doses. In a case of puerperal fever, antipyrin caused a fall of over two and a half degrees Centigrade, with vomiting and diarrhoea. Rigors now came on, the extremities became livid, and in thirty-two hours the patient died in syncope. At the autopsy the spleen was found contracted, the kidneys were shrunken, and contained infarcts. A large number of instances were also collected, in which antipyrin has apparently, in the minds of the recorders, produced disorders so varied and so different from one another that it is scarcely possible for them all to be produced by the same drug.

It appears to us that Eloy is not justified by the facts he cites in reaching the conclusions as to the dangers of the drug which he sets forth. In the case of puerperal fever to which he alludes, nothing occurred which warrants the attributing of the fatal termination to the use of antipyrin, and the post-mortem lesions indeed point to the disease itself as the cause of death, without any aid from the drug.

We think it but a fair conclusion that antipyrin is in no sense a dangerous remedy, and that as yet there are no cases in which death can be attributed directly to its use. All untoward symptoms, clearly produced by any drug, should be reported, but at present the tendency seems to exist of reporting any unforeseen turn in a case as the effect of the drug last administered. Undoubtedly the differentiation is at times difficult, as to whether the symptoms are

of the illness or the drug, but every disease runs at times such an irregular course that the careful clinician thinks twice before charging a drug with a change which his own foresight or care might have averted, or at least expected.

WEIL'S DISEASE.

A FEW years ago PROFESSOR WEIL described a new infectious disease characterized by fever, headache, gastric disorder, jaundice, and pains in the muscles. In the *Deutsches Archiv für klin. Medicin* there have been four or five articles on the subject during the past five years, and in the current number Fiedler, of Dresden, records thirteen cases, and discusses fully the general characters and diagnosis of the disease. The onset of the affection is sudden, without prodromata, and often with a chill. The characteristic symptoms are those above mentioned. The fever runs a typical course, and lasts from eight to ten days. The liver and spleen are usually swollen, and the former is tender. Nephritis often occurs, and herpes and erythema have been present. The cases so far have been chiefly in men, and nine of Fiedler's patients were butchers. The prognosis is favorable. The cause of the disease is unknown, but the fact that dealers in meats form such a large proportion of the cases suggests the direction in which the poison is to be sought. Fiedler discusses the diagnosis, particularly from abortive typhoid, and concludes that it is an affection *sui generis*. It is quite possible that the affection may prove to be a form of chronic ptomaine poisoning.

THE Thirty-ninth Annual Meeting of the American Medical Association will be held at Cincinnati, on May 8th, 9th, 10th, and 11th, under the presidency of Dr. A. Y. P. Garnett, of Washington. The Address in Medicine will be delivered by Dr. Roberts Bartholow, of Philadelphia; the Address in Surgery by Dr. E. M. Moore, of Rochester; and the Address in State Medicine by Dr. H. P. Walcott, of Cambridge, Mass.

The general sessions will be held in the Music Hall, and the sectional meetings in adjoining rooms in the same building. The present indications point to a large meeting.

THE next annual meeting of the South Carolina Medical Association will be held in Columbia, S. C., on Wednesday, April 11, 1888, under the presidency of Dr. T. Grange Simons, of Charleston.

DR. ROBERTS BARTHOLOW, of Philadelphia, we learn, has been elected an Honorary Member of the Royal Medical Society of Edinburgh.

DR. JOSEPH TABER JOHNSON, who has heretofore held the position of Professor of Obstetrics and Gynecology in the Medical Department of Georgetown University, having resigned the Professorship of Obstetrics, the chair has been divided into two separate professorships, Dr. Johnson retaining that of Gynecology, and Dr. P. J. Murphy has been appointed Professor of Obstetrics.

DR. OLIVER WENDELL HOLMES has presented his valuable medical library to the Boston Medical Library Association.

THE New York County Medical Society caused the arrest of an Italian shoemaker for practising medicine without a diploma. At his trial the cobbler entered the plea that he had used no medicines, but simply used charms which had been taught him in Italy, to cure people of witchcraft. His deluded countrymen had to pay roundly for his services whenever they went to him to have the spells of witchcraft lifted from them. He was fined fifty dollars, and he said with a sigh that he would hereafter "stick to his last."

DRS. SAYRE and LUSK, of New York, have been proposed as corresponding members of the Academie de Médecine de Paris.

In an editorial in the *British Medical Journal* of February 25th, on the "Birthplace of Consumption," we find the newly coined verb "to sanitize." The innovation will be welcome to all who have to write upon questions of public hygiene: the evolution of modern civilization makes necessary such additions to our vocabulary.

BUN-POISONING is reported from Belfast, Ireland. A number of children and adults in the workhouse of that town showed symptoms of poisoning after partaking of buns. The medical officer found twenty-three children on the floor vomiting, and some of them screaming. Ten adults had to be removed to the hospital, but all have recovered. An analysis of the remaining buns has been ordered by the authorities.

PROFESSOR NICHOLAS SENN has been appointed Professor of the Principles of Surgery and Surgical Pathology in the Rush Medical College, Chicago.

REVIEWS.

DISEASES OF THE HEART AND CIRCULATION IN INFANCY AND ADOLESCENCE. By JOHN M. KEATING, M.D., and WILLIAM A. EDWARDS, M.D. Philadelphia: P. Blakiston, Son, & Co., 1888.

THE reprinting of the articles on this subject which have run through the numbers of the *Archives of Pediatrics* for the past year will enable the profession to obtain in book form that which they might otherwise have obtained by a subscription to the journal itself, if this is any advantage.

The work of collecting the cases from the literature of the subject must have been most laborious, but we doubt very much whether the present volume fulfils the want which it is supposed to fill—i. e., the information on this subject which the text-books lack.

The printing is clear and well done, and the photographs which are scattered here and there are well executed but almost entirely useless, rather by reason of the difficulty which always surrounds the photographing of pathological specimens than from any lack of skill on the part of the pathologist or artist. Unless each one of them was labelled, we think it would puzzle the average physician to tell what they are supposed to represent.

SOCIETY PROCEEDINGS.

PHILADELPHIA ACADEMY OF SURGERY.

Stated Meeting, March 5, 1888.

THE PRESIDENT, D. HAYES AGNEW, M.D.,
IN THE CHAIR.

DR. DE FORREST WILLARD reported the following case of

SQUAMOUS EPITHELIOMA OF PENIS; AMPUTATION.

A. B., seventy, has had slight burning sensation in the glans penis for nearly two years, but no decided lesion until six months ago, it then ulcerated, and was treated for several months as gonorrhoea and warts, although there had been no sexual congress, other than with his wife.

On examination at first visit to me, a growth nine inches in circumference was found involving the entire glans, which had nearly disappeared. Prepuce not involved, save in the dense induration, which also extended through body of penis almost to the trunk. Whole surface covered with foul pus, and from several points pus was also exuding from corpora cavernosa. Urination painful and difficult. Anterior part of urethra destroyed. No involvement of inguinal or other discernible glands.

Immediate amputation by the knife was performed, close to the trunk, a rubber band controlling all hemorrhage. Arteries tied with catgut, the slit urethra firmly attached to sheath, and skin and parts drawn nicely over the stump. Antiseptic dressings applied, and no catheter used; the parts being thoroughly washed with sublimate solution after each act of urination, and fresh dressings adjusted. Union took place in a few days, and contraction of cicatrix at orifice of the urethra was prevented by use of a silver tube worn while in bed. Subsequently the urine was carried off from the body, when passed, by applying over the stump each time a cylindrical rubber

vaginal speculum. The microscope showed the growth to be of the squamous type. Each corpus contained one or two pus cavities.

The growth will probably return, but an offensive mass is removed, and the next deposit may be intra-pelvic, with a less painful and disgusting death.

DR. WILLIAM HUNT had seen a good many cases of amputation of the penis, but did not recall an instance in which it was necessary to make use of any contrivance for easier urination, or to prevent adhesions, except during the first part of the treatment. It is possible that trouble may have occurred after the patient left the hospital, but he could not recall one coming back to be treated. Cases of penis amputation for epithelioma are very unlikely to have local return of the disease, although this does sometimes happen.

DR. GROSS thought that any surgeon who has operated on many cases of epithelioma of the penis, will find that even if there be no local recurrence, the glands in the groin will, in the majority of cases, be invaded.

THE PRESIDENT said that the question then arises as to the proper method of dealing with the inguinal glands in these cases. Would it not be as important to remove these glands in cases of epithelioma of the penis as to remove the axillary glands in cases of carcinoma of the mamma?

DR. GROSS thought that the importance of this is recognized more particularly by the German surgeons, who are certainly more in advance of the surgeons of other nations as regards the extent of operation that should be performed in carcinoma. They remove the glands in the groin. There are many cases, however, in which, although the superficial glands be removed, the deep glands in the pelvis will be found involved. This is more apt to be the case when the glands in the groins form a large mass rather than a distinct enlargement of the separate glands. In many cases the glandular involvement has gone so far that operation is out of the question.

DR. W. W. KEEN read a paper on

THE RADICAL CURE OF HYDROCELE BY EXCISION OF THE TUNICA VAGINALIS TESTIS.

(See page 371.)

DR. GROSS said that he was the author of the article in THE MEDICAL NEWS to which Dr. Keen had referred. Since he wrote that paper he had, from time to time, noted some other points in regard to operations for hydrocele, which may be of interest. Some years ago, Dr. Allis made statements to this body which surprised the most of his hearers, concerning the various methods of treatment of hydrocele, and he showed that none was absolutely safe, not even that of the injection of various mixtures and solutions. The records of a larger number of cases show that the conclusion of Dr. Keen with reference to excision is correct.

He might correct him as regards the first excision of the vaginal tunic. The history of the operation is briefly as follows. In 1873 Volkmann resorted to incision of the tunica vaginalis, with drainage and suture of the edges of the tunica vaginalis to the edges of the scrotum. In 1878 he incised the tunica vaginalis, united it with sutures, and also passed deep sutures, bringing the surfaces of the tunica vaginalis together. In 1882 he excised a large portion of the vaginal tunic, sutured the remainder with

gut, and united the scrotum with silk, putting in two drains. He was, therefore, the first to excise, as he was the first to incise. Juillard, of Geneva, in 1883, excised all of the vaginal tunic except a sufficient portion of the parietal layer to bring over the testis and epididymis, and thus shut up the testis in a smaller sac of the tunica vaginalis. He inserted a drain in the scrotal cavity, and united the scrotum with catgut. Nicaise strips off the tunica vaginalis, saving enough to cover in the testis, and uniting this with catgut he puts in a small drain. He then unites the scrotal wound and puts in a second drain. A year or two ago, the speaker himself excised the tunica vaginalis, and then approximated the sides of the scrotum with the figure-of-eight suture of Heppner.

Now a word with reference to statistics. He had collated 540 cases of iodine injection from the records of later years, particularly by German surgeons. There have been no deaths, but there was recurrence in 8.15 per cent., and acute suppuration or gangrene in 1.66 per cent. Of 90 cases of carbolic acid injection, all were successful. In three cases, however, there was suppuration, but in all from one to three drachms of carbolic acid had been used. Still smaller quantities may, moreover, give rise to suppuration. He had seen, in the practice of a colleague, suppuration follow the injection of half a drachm into the left side of the scrotum, and Abbe has had suppuration follow the injection of only twelve minims. The percentage of suppuration after carbolic acid injection, 3.33 per cent., has been greater than after the injection of iodine. In 305 cases of antiseptic incision there was recurrence in 4, and he believed that that was the reason that Volkmann resorted to excision. There were also 4 deaths, 1 from pyæmia, 1 from septicæmia, 1 from tetanus, and one was said to have died of phthisis. It may be remarked that every one of these cases died under the carbolic acid dressing. There has been no death reported, so far as he knew, from incision since the use of corrosive sublimate dressing. As Dr. Keen had remarked, there is frequently in these cases of Volkmann's operation, orchitis, occasionally abscess, and partial sloughing of the edges of the wound. We now come to the 78 excisions; of these one recurred but none died, and there were no subsequent complications, so that in reality the safest and most satisfactory method of treating hydrocele reported up to the present time is excision of the sac. It may be that there has not been a sufficient number of cases, but the fact that there has been only one recurrence and no deaths or accidents, would indicate that in suitable cases this is a proper operation.

In an ordinary hydrocele, he should not at once resort to excision. In the case of a hydrocele containing, for example, eight ounces, he should empty the sac, and when it had refilled one-third, inject some solution, as there would then be less surface to be acted upon. In a case that has never been operated upon he empties the sac and tells the patient to return when it has refilled one-third. He then throws in thirty drops of ninety-five per cent. commercial carbolic acid. If the patient has had injection practised, and particularly if the operation has been repeated several times, he thinks that we should at once proceed to open the sac. In these cases we will find that the vaginal tunic and the fibrous tissue under it have undergone changes. The vaginal coat is not only thickened and ecchymosed in spots, but in places it has

undergone cartilaginous degeneration, and is frequently beset with cysts or other abnormal products.

He has great confidence in the treatment of hydrocele with carbolic acid. He has practised it for some years and has never had a failure from it nor has he seen any bad results, although, as he had stated, one of his colleagues had a case in which suppuration followed its use, but this must be attributed to infection by the instrument.

DR. J. EWING MEARS was not aware that the method of treating hydrocele by excision of the tunica vaginalis was so recently instituted. He performed it himself in 1876, not knowing that it had been so rarely undertaken. This was in a case of old hydrocele which had been operated on a number of times. The tunica vaginalis and the subjacent tissue were much thickened, and there was also atrophy of the testicle. He not only removed the tunica vaginalis, but also the atrophied testicle, which was not larger than an almond. The wound was dressed antiseptically, and the patient recovered without a bad symptom.

He thought Dr. Gross had indicated the proper method of treating these cases, and that all are agreed that in simple hydrocele, not previously operated upon, the injection of some fluid—Dr. Gross suggests carbolic acid, he had used iodine with success—is the proper plan. He injects the iodine after the method suggested by the President, evacuating the contents of the hydrocele, then injecting the iodine and allowing it to remain. He has never had any unfavorable symptoms follow its injection in this way, nor the persistence of severe pain, as stated by Dr. Gross. He thought that it is the operation most likely to succeed in simple cases. In old cases of hydrocele with thickening of the tunica vaginalis, there is no doubt that excision is the only remedy that will give a radical cure.

DR. GROSS said that excision is not the only remedy that will bring about a cure when the tunica vaginalis is thickened. The first case in which he did Volkmann's operation was one in which there were patches of cartilage in the vaginal tunic. He did not excise it, but the cartilage reverted to embryonic tissue and disappeared. The only disadvantage was that the cure was protracted. It was about three weeks before the incision had healed. Of course, excision is the best, although not the only operation in these cases.

DR. JAMES M. BARTON referred to a method which he had been using with considerable satisfaction for the past three or four years. He had met with a number of cases of hydrocele which had resisted treatment. It was formerly the method in Jefferson College Hospital to treat these cases with a seton. This was often followed by unpleasant results. Finding that these were, to a large extent, due to insufficient drainage, he began by introducing a drainage tube at the lower opening at the time of operation, subsequently substituting a drainage tube for the seton. This answered well, and he thought was still further improved by making the operation antiseptic. He had treated ten cases in this way with success. The parts are scrubbed with turpentine, water, etc., on the day before the operation, and covered with a piece of cheese-cloth saturated with a solution of the bichloride of mercury. Over this is placed a piece of rubber cloth, a hole being made for the penis, and the dressing held in position by adhesive plaster. The following day the trocar and canula is entered at the lowest possible point, the

sac emptied, and the instrument brought out at the highest point. A drainage tube, somewhat larger than the canula, is carried through, the tube being stretched to reduce its calibre. The parts are then covered with antiseptic gauze. In the course of from two to four days, lymph will appear around the tube, forming a mass about one inch in diameter. At the expiration of the above time the tube is removed, and the patient allowed to go about. He had in most cases succeeded without inducing suppuration. In one or two cases there has been slight suppuration, but the only disadvantage resulting from this has seemed to be the delay in recovery.

DR. KEEN desired to correct one error into which Dr. Gross had fallen. He stated that there had been no deaths since the bichloride of mercury had been introduced. Dr. Bull has reported one case of death from mercuric poisoning after Volkmann's operation. He was glad to see the general impression with reference to the difference in danger between complete excision and the incision of Volkmann. Certain it is that the method by incision is not by any means so radical, so devoid of danger, or so philosophical as the entire removal of the tunica vaginalis, with the exception of that portion covering the testicle and cord. He agreed with those who had spoken that excision is the best treatment for cases of old hydrocele which have been repeatedly operated upon; and from his experience with the methods of injection which are followed by so much of induration, of swelling, of weight, of inflammation, and of pain, he was very decidedly inclined, even in cases of recent hydrocele, to practise extirpation of the tunica vaginalis. It is well known how aseptic and non-inflammatory the course of such wounds is. It seemed to him that we should have much better results in the future than we have had from carbolic acid injections in the past.

DR. WILLIAM HUNT asked if there had been any observations with reference to the behavior of the testicle after successful excision of the sac for hydrocele. It has been well established that in the ordinary operation the cure is not due to the adhesion of the two sides of the sac, but to an alternative action exerted by the injected solution upon the membrane. Now, after the sac has gone, does the testicle adhere to the connective tissue, and become restricted in its movements, or is a lubricating surface or pseudo-cyst formed around it?

DR. WILLARD asked if the contraction might not destroy the functional activity of the organ?

DR. KEEN was not aware of any observations upon these points. He took it for granted that there would be complete adhesion between the two surfaces. He thought that no contraction would take place, and that the function of the organ would not be interfered with.

DR. GROSS said that, going back to the subject of injections, the reason that he adopted carbolic acid was, because it is followed by so little inconvenience. After throwing in thirty drops of commercial carbolic acid the pain ceases in a minute or two. Tincture of iodine, or any of the preparations of iodine, on the other hand, causes excessive suffering which may last for hours.

DR. J. HENRY C. SIMES said that he had used injections of cocaine previous to the injection of iodine, and found it very satisfactory in preventing pain.

DR. D. HAYES AGNEW still adhered to tincture of iodine, but occasionally used carbolic acid. He thought that we cannot tie ourselves down to any one method of

treatment. In most cases iodine or carbolic acid will answer. He still preferred tincture of iodine, but where there are extensive changes in the sac it is proper to excise. With reference to the pain following the injection of iodine, his experience does not agree with that of Dr. Gross. The suffering is intense for two or three minutes, certainly not more than five or ten minutes. He keeps the patient quiet for five or six days, after which he is allowed to go about with a suspensory bandage. He thought that we cannot draw conclusions with regard to the value of iodine injections from statistics, because iodine is used in such a variety of ways. Some dilute it, while others allow it to escape immediately. To make a success, pure tincture of iodine must be thrown in (never less than two drachms) and allowed to remain.

DR. WILLARD had not seen long-continued pain follow the injection of tincture of iodine. He had seen it continue two hours, but usually not longer than from ten to thirty minutes.

MULTIPLE FRACTURE.

DR. HUNT wished to refer to a case, and his object in doing so is to put it on record. It is a curious one of multiple fracture. A man, forty-one years of age, came into the Pennsylvania Hospital after falling a distance of ten or fifteen feet striking the cobbles. The injuries were confined to the right arm. There was a fracture through the neck of the humerus and dislocation of the head of the bone, which was reduced; there was a fracture of the olecranon and a Colles's fracture.

DR. PACKARD thought that this was an unique case. The man has been in the hospital about two weeks and there is nothing remarkable in the progress of the case. What forces caused the combination of injuries without breaking the shafts of the bones anywhere, is hard to determine. For a man to break the same limb at all three main joints at the same time, is certainly a very rare occurrence.

NEW YORK ACADEMY OF MEDICINE.

SECTION ON ORTHOPÆDIC SURGERY.

Stated Meeting, March 16, 1888.

V. P. GIBNEY, M.D., IN THE CHAIR.

DR. SAMUEL KETCH read a paper on

THE PRESENT STATUS OF THE TREATMENT OF ROTARY LATERAL CURVATURE OF THE SPINE.

This condition, he said he felt sure, had given rise to more theories and, as a result of these, more ingenuity in the means devised for the relief of a deformity presenting greater obstacles to success, than any other encountered by the orthopædic surgeon. In speaking of rotary lateral curvature of the spine he wished emphatically to be understood as referring here to only typical cases, and he defined such rotary lateral curvature to be that lateral deviation of a portion or whole of the vertebral column which shows as its most constant factor the rotation, torsion, or twisting of the bodies or segments of bodies of the vertebræ, with accompanying or consecutive alterations in the shape of the thorax. No deformity with which we are brought in contact exhibits in its slow, insidious progression, in its involvement of area, in its secondary effects on important internal organs, so decided alterations of form, and such marked disturb-

ances of function, as this. When it becomes recognized that the condition we have to deal with is but the effect of some cause, some irritation in fact, the exact nature of which is as yet unknown, it will be seen that the problem of treatment embraces something more than the attempt to remove the mechanical results of such cause by the use of opposing mechanical means.

The present treatment of the disease consists principally of two methods, or a combination of the two, viz., by apparatus and by exercises, or by the conjoined use of both. Dr. Ketch did not think it necessary to speak in detail of the treatment by complete recumbency or that by cutting of spinal muscles (tenotomy), although both plans, he thought, might be useful adjuncts at times. He went on to say that at the present day apparatus for the relief of lateral curvature might be described under two varieties, progressive and fixative. He strongly condemned treatment by the constant action of cumbersome and heavy instruments which by their abnormal pressure often give rise to large excoriations, and even sloughs of the skin. In his opinion no lever brace should be used for any other purpose than simply that of retaining the spine in an improved position previously obtained either by appropriate manual exercises or combined manual and gymnastic movements. Such braces should be light, easily removable, to allow of inspection of the parts, and simple enough to be easily applied by those having the home care of the case.

The second class, or fixation apparatus, were originally constructed with the idea of firmly immobilizing the distorted spine in an improved position, obtained by suspending or extending the patient. This original plan has been modified to the extent of opening the brace in front, whether constructed of plaster-of-Paris, poroplastic felt, leather, or other substance, with the object of easy removal, in order to allow proper exercises and give attention to the subjacent parts.

In regard to the matter of exercises he said that he attached most importance to those which have for their object the "unfolding" of the twisted vertebral column. These can be applied with the greatest precision and benefit by means of the surgeon's hands. The importance of manual force has not been overlooked by surgeons in their attempts to relieve deformity, and no appliance can rival the ease, proper application of pressure, and safety which manual exercises afford. Their success, however, depends on detail, and they should be given either by the surgeon himself or a thoroughly trained assistant.

As to what is meant by a cure, Dr. Ketch said that to his mind this would involve not only the restoration of the lateral deviation, with its normal curves, but also the disappearance of that most constant factor of the deformity, the rotation. As generally understood, however, this implies either the prevention of further deformity or an amelioration of the condition as originally observed. In the cases observed by him he has never been able to convince himself of the entire disappearance of rotation, while he believes it to be essential that this symptom should be prevented or removed before a vertebral column can be considered as restored to a perfectly normal state. To the question, Are we able at the present day to remove this rotation? he felt compelled, then, to answer in the negative. He then took up the question, Are there any means by which we can more fully accom-

plish our ends? In the way of apparatus, he said, there is nothing new, for all apparatus finds its efficiency in the proper application of mechanical laws. In the way of prevention, however, a great deal can be accomplished. It is only by the earliest recognition of the symptoms and an appreciation of their importance that prophylaxis can be accomplished. He then referred to a paper which he read two years ago before the Section on lateral curvature, with special reference to its occurrence in children. It was based on an analysis of 229 cases presenting the typical deformity in question, and he divided these cases into three classes, as follows:

1. Those where the deformity was first observed from the first to the twelfth year, or the age of childhood.
2. Those where the deformity was first observed from the twelfth to the eighteenth year, or the age of puberty.
3. Those where the deformity was first observed from the eighteenth year and upward, or the age of complete development.

During the period of childhood the deformity occurred in 120 cases, or over one-half of the whole number, and among the conclusions at which he had arrived in the paper were the following:

1. Rotary lateral curvature is principally a disease of childhood, and may be either congenital or acquired.
2. The disease should be looked for early in life, and as a factor in early treatment the careful inspection of children's spines becomes most important toward the prevention of the deformity.

He did not mean to say that even were the spines of young children to receive the utmost care, and at the first suspicion of a deformity the child was taken to a surgeon, that we should not have lateral curvatures; but he felt convinced that the inveterate deformities now seen in so large a number would be few and far between.

DR. A. N. PHELPS said that in lateral curvature of the spine, if there were changes in the bone, no apparatus or gymnastic exercises would correct the deformity. It was, however, of advantage to suspend the patient, and, having got him into the best possible position, to secure this permanently by some fixed apparatus. He believed Stillman's brace to be the most satisfactory steel instrument that had been devised; but, so far as his experience went, he had found that the plaster-of-Paris jacket was by far the best appliance at the command of the surgeon. He thought that compensating curves could be maintained which would be of service in mitigating the deformity.

DR. A. B. JUDSON believed that mechanical treatment which involved strong lateral pressure on the ribs, would increase, rather than reduce, the rotation and curvature. If we could invade the cavities of the chest and abdomen, and make lateral pressure on the errant vertebral bodies, it would be easy by direct force to correct at one motion both the curvature and the rotation. His views of treatment were based on the importance of placing the patient as often, and for as long a time as practicable, in those positions in which the deformity was diminished or entirely reduced, so that, as the patient's frame developed, the skeleton might grow and harden with as little deformity as may be. As the superincumbent weight is the direct cause of the curvature, and as the deformity disappears in mild cases when the patient lies down, the recumbent position should be maintained at least two or three hours in the middle of the day. And if general debility, or fatigue, or muscular failure is to be

considered the indirect or remote cause of the affection, something might be gained in this direction by the prescribed recumbency as well as by encouraging the patient to avoid fatiguing tasks, and to lie down whenever so disposed. Again, when the patient is lying down he considered it advisable that he should remain on his back, and not only on his back, but in such a position that a lordosis is produced. This was accomplished by placing an air pillow, blown up to its full extent, under the spine. The reason for this is, perhaps, a little theoretical. In the rotation of the spinal column it is clear that the anterior portion of the vertebræ is the most at fault. The same is true in Pott's disease, the disease being located in the anterior portion of the vertebræ, and, therefore, in Pott's disease also, is produced a lordosis, if possible. We remove the pressure from the anterior part of the vertebræ to the posterior.

Another very useful position was that in which the weight of the body is suspended by the upper extremities. It is well known that even in cases in which the deformity is marked the curvature and rotation absolutely disappear when this attitude is taken. He referred to the theory of Dr. Henry G. Davis, who pointed out that in this position the chest is directly and forcibly enlarged by the fixation of the anterior ends of the ribs, which are connected by the fibres of the pectoral muscles with the humerus, while the posterior ends of the ribs are dragged down by the weight of the lower part of the body. Suspension may be practised by the rope and pulley, the suspension bar, the parallel bars, the overhead ladder, or the suspended rings. As much exercise of this kind as is possible should be taken short of fatigue, with the threefold object of maintaining an improved position of the spine, of increasing the capacity of the chest, and thus indirectly combating rotation, which narrows the chest, and of securing the improvement in general health and strength which naturally goes with improved respiration. He did not believe that the cases of extreme deformity, which are so common, could be materially improved in shape, but some cases of moderate severity can be entirely restored to symmetry. He had never seen a case pass from the moderate to the extreme type of deformity.

DR. REGINALD H. SAYRE agreed most thoroughly with Dr. Ketch, in saying that preventive rather than curative treatment is what we must aim at in lateral curvature. He also bore out his observations as to occurrence in early life, having seen one case where the curvature is said to have been present at birth, another where it was noticed at six months, and having personally seen a case in a child two months old.

As for getting a perfect restoration to the normal form, he believed it to be possible only in cases where there is but very slight rotation, cases which present considerable rotation and curving of the ribs he believed can never be restored completely to the natural shape. They may, however, be benefited, and he had been surprised to see the amount of improvement attainable in very advanced and, at first sight, apparently hopeless cases.

If possible, he would treat a case by gymnastics and massage alone, with the object of educating the muscles of the patient and enabling him to retain such improvement of position as he could make by manipulation, and exercises, which, being different in various cases, would require too much time to be described here.

If a case is sufficiently distorted to require also mechanical support, or if it is necessary, on account of paralysis, he should use a plaster-of-Paris corset, as he has never seen a case which became worse under this mode of treatment, and he has seen a number of most frightful distortions occur under instruments of various kinds, applied while the deformity was very slight, as attested not only by the testimony of the parents, but by photographs taken at the time of beginning treatment; and as this result has followed instrumental treatment in the hands of various physicians of large experience, with patients whom he has found most obedient in following out instructions, he is forced to conclude that the fault lay in the method.

His patients, who have worn both braces and plaster-of-Paris jackets, all give testimony in favor of the superior comfort of the latter, which would lead him to use this method even were the results no better than those attained by instruments, but where he sees improvement take place in patients who have steadily grown more crooked during years of instrumental treatment, he feels more and more that most of the machines he has seen are worse than nothing.

DR. RIDLOW thought that surgeons are not sufficiently definite as to what is meant by cures and what by exercise for the patient. He was in the habit of dividing the cases which he saw into three classes for prognosis and treatment. In the first class there was little or no rotation, and the spine could be put into what seemed to be the normal position for a brief space of time. In the second class there was more or less rotation and rigidity of two or more segments of the spinal column, and the deformity could not be reduced, but pain and weakness were absent. In the third class there was marked curvature and rigidity; and, in addition, pain and weakness.

In the first class of cases he believed that a cure could be effected; and here he deprecated all use of apparatus in the treatment. In the second class of cases he was inclined to think, with Mr. Bernard Roth, of London, that the spine could be made straight and the rigidity overcome. Gymnastic exercises are of great service, but as to mechanical means, he did not feel sure that they are of any value. In fact, he was convinced that they sometimes do harm, but at the same time he found it advisable to employ them himself in certain instances. Any apparatus made use of should be light and elastic, and made to fit. In the third class the spine could not be straightened or the rigidity overcome, but the patient could be made to carry himself in better form. Here he was accustomed to prescribe certain exercises, and in addition he used a steel apparatus which would act somewhat as the hand of the surgeon in correcting the deformity. It should not be flexible, but he thought it should be changed in its adjustment each day according to circumstances. The osseous curve, in his experience, always remains unchanged. Of course, much can be done by baths, massage, and electricity; but more can be accomplished by the use of suitable gymnastic exercises than by any other means.

He said he was surprised that something more definite in regard to such exercises had not been insisted upon in the present discussion. Sitting upon a stool and twisting the body he thought accomplished as much as anything else in developing the spinal muscles. The patient

should be put in a position nearest the normal that was attainable, and instructed to maintain this to as great an extent as possible. Dr. Ridlow then went on to advocate the system of exercises described by Mr. Roth in Heath's *Dictionary of Surgery*, the "key-note" position (in which the patient is to be exercised) being that position of the trunk and arms in which the greatest improvement in the position of the spine is obtained. A cure was considered by Mr. Roth to have been accomplished when the patient was so educated that the best possible position became an habitual one. This surgeon, he believed, had done more to secure good results in lateral curvature than any one else that he knew of. If we knew just what we could accomplish in any particular case, a definite plan of treatment could be adopted, which is much preferable to the uncertain and slipshod methods hitherto in vogue.

DR. V. P. GIBNEY stated that he had not much to add to what Drs. Judson and Ridlow had said. He was now hard at work in practically testing Roth's system of treatment. Among the points which had not been taken up in the discussion was the subject of measurements, and he thought it of the utmost importance that some definite system should be adopted upon which all orthopaedic surgeons could agree. In carrying out Roth's methods it is very difficult to get dispensary patients to come to the surgeon every day, and he had recently organized an evening school of gymnastic exercises for young girls afflicted with lateral curvature. Their powers of resistance seemed, as far as he was able to judge, to have developed wonderfully already, and the experiment bade fair to be a very successful one.

In closing the discussion, DR. KETCH said he thought that it would no doubt be as difficult to maintain the compensating curves spoken of by Dr. Phelps as to remove the lateral curvature and rotation. He could not say that he regarded a rotary lateral curvature cured which had a deformity remaining. Braces will either prevent the deformity from becoming worse, or might tend to produce amelioration. Experience with special kinds of apparatus enabled some surgeons to obtain better results with these than other surgeons could who had not the same facility with them. Thus, Dr. Sayre was no doubt more successful with the plaster-of-Paris jacket than those less familiar with this appliance. A steel brace should have a fixed point for making pressure, which should be oblique, and not directly lateral. In regard to Dr. Judson's remarks, he thought that any analogy between inflammatory disease of the vertebrae and rotary lateral curvature was inadmissible. It seemed to him that Dr. Judson had been especially fortunate in not having met with any of those inveterate cases referred to in the paper and the discussion. There is no deformity so much like that of clubfoot as rotary lateral curvature of the spine. The former condition, however, is usually detected soon after birth, and measures taken to correct it. Hence we met with much fewer cases of inveterate clubfoot than of lateral curvature, since the latter is not, as a rule, treated at a sufficiently early stage. After all, the prevention of deformity was the point on which he desired to lay most stress. He believed that no two cases are alike, and whatever the plan of treatment adopted frequent inspection and the most careful watching are essential on the part of the surgeon.

SECTION IN PEDIATRICS.

J. LEWIS SMITH, M.D., CHAIRMAN.

Stated Meeting, March 28, 1888.

DR. A. JACOBI read a paper on

SEPSIS IN THE NEWBORN.

Semmelweis was the first to teach that puerperal fever in women and newborn infants is of traumatic origin, septic material being absorbed through erosions and abrasions of the skin and mucous membranes, the lesions in the newborn being especially those which occur about the head and navel. The changes in such cases are often manifested in the umbilical vein, arteries, and surrounding tissue; and they sometimes occur in cases in which thrombi are, to all appearances, normally formed. Some writers have stated that it is impossible for sepsis to occur through the cut end of the cord, excepting in cases in which the ligature surrounding the stump is insecure, but Budin has seen cases in which pus could be expressed though the ligature was properly applied, and the author had also seen such cases in which the umbilical wound refused to heal.

The question may be asked, Whether sepsis can occur without apparent disturbance of the navel? And the author would reply that this is possible, though it is not of frequent occurrence. Cases undoubtedly occur in which a newborn infant is infected while the mother remains unaffected, the poison being transmitted by the discharges from the mother; or the fetus may be infected through the medium of the maternal blood. In such cases no wound surface in the maternal tissues may be apparent. Sepsis may also occur, according to some writers, through the mother's milk; but if this is possible it is also true that many infants are so robust that they remain unaffected however contaminated the milk may become. Septic pneumonia and other pulmonary troubles have also been recorded in consequence of the inhalation and aspiration of decomposed or decomposing material. If there are abrasions and erosions of the skin, sepsis may result even though the lesion may apparently be a very slight one. Spontaneous pyæmia has been described by certain authors, and though such a supposition seems irrational, cases occur now and again in which no lesion can be found. Two such cases were recently seen by the author.

Ebstein describes three forms of sepsis in infants which seem to be due to lesions in the mouth: (1) Septic catarrh which may result in hemorrhage and other accidents, (2) croup, and (3) diphtheria. In such cases there are unquestionably lesions of the epithelium of the oral cavity. These lesions may be slight, but are yet sufficient to afford an avenue for the entrance of septic elements. The practical point in this matter is that manipulations about the mouth of the newborn infant should be of the gentlest possible character. Lesions of epithelium in the vagina and anus may also give rise to sepsis.

Pneumonia in the newborn is not of infrequent occurrence, though it occurs more frequently after the third or fourth day of life. It may result from the aspiration of liquor amnii, meconium, etc. Too much stress should not be laid upon the influence of micrococci. They

occur in all parts of the body, and the micrococcus of sepsis is not yet absolutely determined. Much yet remains to be done upon the bacteriology of this subject. Experimental pathology is useful, but it cannot prove everything, and much of it is exceedingly faulty. To the question, Whether sepsis can be transmitted by the air? it may be replied that it can, even without the evidence which is afforded by experimental pathology. The symptoms and localization of this condition are many and varied—for example, dermatitis of varying intensity, syphilis, ulcerations of the scalp and of the umbilical stump. Hemorrhage, which is a very common symptom, may be due to the influence of micrococci, or it may be due to the undeveloped condition of the vessels in the newborn infants which predisposes them to give way from very slight causes. Purulent metastases also occur frequently in different parts of the body, or the inflammatory collection may consist of bloody serum or sero-pus. It must not be forgotten that changes in such patients take place very suddenly, and often with a fatal issue, when no great rise of temperature has suggested a serious condition. Great loss of weight is apt to be noticeable in such cases. If the case becomes chronic, the chances of recovery become more favorable.

DR. J. LEWIS SMITH read a paper in which cases were narrated showing

THE MANNER IN WHICH SEPSIS OCCURS IN THE NEWBORN.

He had observed that this is sometimes obscure, but that the poison usually enters the system through the umbilicus, and is usually microbic. The cases which had come under his observation were divided into groups: First, those in which an umbilical phlegmon had resulted from the umbilical ulceration. In some of these cases diphtheritic inflammation was present. This was especially the case in connection with an epidemic which recently occurred at the New York Infant Asylum. In other cases the inflammation of the umbilicus was accompanied with sepsis of moderate intensity. In the epidemic referred to the disease seemed to be due to the poisonous atmosphere of the wards of the hospital. Careful examination of the air revealed the presence of multitudes of disease germs, and even after the wards had been fumigated with sulphur, disease germs could still be found and cultivated. In this group of cases the infection was transmitted mainly through the lymphatics. The pathological investigations upon the cases which were reported were made by Prof. T. Mitchell Prudden.

The second group included cases in which the poison was transmitted through the umbilical vessels, especially the vein. These had not been so numerous as the first group.

The third group included cases which were infected by all other possible channels, as the lungs, the skin, etc. If sepsis is possible through the umbilicus, as seems to be proven, there is great probability that it may also occur, if less frequently, through other channels.

Trousseau has observed that infantile erysipelas is most frequently seen in hospital wards in which puerperal fever is present. Such cases would now-a-days be considered of septic origin. Fortunately, they are more rare than they used to be, and this is probably due to improved methods in obstetrics.

DR. ANDREW F. CURRIER read a paper entitled

A RÉSUMÉ OF RECENT VIEWS CONCERNING ICTERUS NEONATORUM; AND ITS RELATION TO SEPSIS.

The great size of the liver and its consequent capacity for holding blood; the abolition of the umbilical vein and ductus venosus, the channels by which the placental circulation is maintained, and the degenerative changes which occur in the blood after the ligation of the umbilical cord are facts which enable us to understand why morbid conditions, which are traceable to the liver, occur so frequently during the first few days of life. A review of the various theories which have been advanced to explain the etiology of icterus neonatorum shows great differences of opinion, but the following facts seem to be pretty well established: (1) With the completion of foetal life and the termination of the placental circulation, extensive changes in the blood of the infant always take place. (2) These changes are degenerative and are much more marked in immature and feeble infants than in those who are robust. (3) The external evidence of these changes appears in the form of icterus, which, in the greater number of cases is of brief duration, and is not accompanied by symptoms indicating serious constitutional disturbance. (4) In those cases in which the icterus is profound and persistent, serious results are to be expected, for the condition is not caused by the ordinary degenerative changes in the blood corpuscles which always follow the ligation of the umbilical cord. (5) Among the causes which are followed by severe or fatal symptoms are (a) Non-closure of the *ductus venosus* (Quincke's theory). (b) Defects and malformations of the bile ducts. (c) Oedema of Glisson's capsule (Birch-Hirschfeld's theory). (d) Weakness of the heart, especially its right side, pulmonary atelectasis, insufficient anastomoses of the veins of the umbilicus with the contiguous veins of the abdomen, syphilitic and other new growths, malformations and deformities of the liver, and other causes which lead to insufficient tension in the subdivisions of the portal vein, with consequent stasis, effusion of bile and resorption of the same into the circulation either directly by way of the hepatic veins, or indirectly by way of the lymphatics.

The relation of icterus to sepsis depends largely upon the conception which one has concerning sepsis. If we understand by sepsis the disturbing influence which is produced upon the body and its functions by microbes of decomposition which are conveyed to it from without and through the medium of a wound surface, there are undoubtedly certain cases of icterus in which sepsis plays an important part. The ordinary cases of icterus in which there is no particular disturbance of function are simply the result of hemoglobinæmia, which Silbermann considers physiological within certain limits. There are other cases in which the icterus is more profound, and the evidences of disturbance in the secretions more decided, but nothing which indicates the presence of pus in the system. The umbilicus in such cases has a healthy appearance, and there are none of the ordinary symptoms of phlebitis though the cases may proceed to a fatal issue.

In a third class of cases the skin may or may not be decidedly yellow, there is great constitutional disturbance with high temperature, dry tongue, and tender and swollen belly, there may also be chills and abscesses in the

parenchyma of the liver and elsewhere. The umbilicus is swollen and ulcerated, and pus or blood exudes from the arteries or veins, or both. Birch-Hirschfeld found in the blood of such patients both rod and spherical bacteria. Schüller and Vogel observed that this condition coexists with puerperal fever in the mother, and it is not improbable that the same element of infection is common to both. Between such cases, in which there are manifestly septic elements and conditions with more or less decided jaundice, and those in which the jaundice is due to an excess of hæmoglobin in the blood or to a resorption of bile, a clear and broad distinction can be made. The former are simply cases of septicæmia, the latter are cases of toxæmia, in the sense that the blood either contains a substance which is abnormal to it (the bile), or an excess of a normal element (the hæmoglobin).

CORRESPONDENCE.

COCAINE POISONING AND ITS ANTIDOTE.

To the Editor of THE MEDICAL NEWS,

SIR: I submit to you an interesting example of cocaine poisoning which came to me for treatment last fall. I was called up to attend the following case of cocaine poisoning in the person of a student who had been experimenting on himself by hypodermatic injections of a four per cent. solution of cocaine at regular intervals during the day, but they were not followed by its toxic influence until some four hours after its last exhibition. These were shown principally by irregularity of the circulation accompanied by cerebral excitement.

Patient's attacks came on in paroxysms whose onset were indicated by a steady, constant rise in the pulse accompanied by tinnitus and a sensation as though the head would burst. Capillary circulation somewhat interfered with, the extremities being colorless. At the height of the paroxysm patient grasped the head between the hands and labored under intense excitement lasting from thirty to sixty seconds, when there was a gradual decline in the pulse to normal (and sometimes below) and also of the cerebral symptoms, patient becoming quiet during the interim which lasted from one to two minutes, when the same cycle of symptoms were repeated. Pupils and tactile sensibility normal, respiration not markedly interfered with except at the height of the paroxysm, when it became more superficial and hurried.

Amyl nitrite seemed clearly indicated, and was used by inhalation with immediate relief, followed in a short time by an entire disappearance of all untoward symptoms.

Yours very truly,

F. P. EMERSON.

CHESTER, VERMONT, March 18, 1888.

NEWS ITEMS.

CINCINNATI.

(From our Special Correspondent.)

The American Medical Association.—A large attendance is anticipated for the approaching meeting of the American Medical Association at Cincinnati; that it will be of interest and profit to its members is confidently expected. Cincinnati is especially adapted by location and her

excellent railroad facilities to the reception of the Association. The city is easy of access from every quarter; and her hotel accommodations are abundant. The Committee on Transportation report that it has secured the customary reduction of fare (full rates to the meetings, and one-third rates returning) over all roads.

The Committee of Arrangements, under the efficient leadership of Dr. W. W. Dawson, has been for some time actively engaged in preparing for the event. The great Music Hall has been selected as the place of meeting, as it affords ample facilities for the holding, not only of the general sessions, but of all the sections as well. It is also easy of access from all the hotels, and by street railways from every part of the city.

As is usually the case, the titles of papers to be read are not promptly reported, and the programme is still very incomplete. It is, therefore, impossible to judge, as yet, of the amount or character of scientific work to be done.

One of the most interesting features of the meeting will be the exhibition of drugs, books, surgical instruments, etc. In this feature the meeting is expected to excel. This is due chiefly to the fact that arrangements have been made to retain many of the exhibits for the Centennial Exhibition of the Ohio Valley and Central States, which begins here on July 4th.

The entire programme for the entertainment of visitors has not yet been made known. It is generally understood, however, that there will be a large reception at the Art Museum, and a grand concert at the Music Hall. Every effort will be made to render the visit of the Association enjoyable, without losing sight of its scientific interest. Nothing is more desired by the profession here than that the coming meeting should strengthen the bonds of friendship between the members of the Association, and it is hoped that the meeting in May will be memorable for the large number of men coming from every quarter of the country who have at heart the well-being of the Association, the interests of the profession, and the advancement of medical science.

Bandage Cases in Switzerland.—Col. Ziegler, physician-in-chief to the Swiss army, has introduced the use of a dressing or bandage case which will be exceedingly useful to the soldier in the field. Captain Secretan has lately given a very interesting lecture on this subject, in which he says: "Col. Ziegler, with the aid of Dr. Kocher, of Berne, has had made some very practical small cases. They contain a muslin bandage, about six feet in length, in a wrapper of parchment paper. There is sufficient gauze to make a provisional antiseptic dressing for wounds of small dimensions; for instance, such as those caused by bullets. This is very important, as every one knows these wounds are less dangerous the sooner they are protected from infection. It was at first intended to distribute these cases to the men, as is done in Germany, where each soldier is provided with a small box of dressing, sewn into the flap of his tunic between the lining and the cloth; but practice has shown the evils of this system. Nine times out of ten, when the dressing was needed, it was found soiled by perspiration and dust, deteriorated by damp, or that the soldier had used it for other purposes. Therefore, these cases will not be distributed to the troops. They will be wrapped up in packets of five in an envelope of waterproof paper

bearing instructions as to their use, in three languages, and they will be kept in the store. The doctors will have them in their cases in reserve, the ambulance corps in their knapsacks, and they can be ultimately distributed to the patrols, to guards, and to isolated detachments where medical care might momentarily fail."

A Double Monster.—The *Madagascar Times* reports that a Malagasy woman in Antananarivo gave birth to a double female child. The child had four eyes, two noses, four ears, four arms, two mouths, and four legs, but only one head and one body. Both parts of the child were female. Both the mouths uttered sounds, but only one suckled the mother. The sight was so strange that the babe was put to death on the second day, and the mother, we are told, is severely ill from fright.

A Lack of Military Surgeons in France.—A recent competitive examination for the French army medical service failed to attract a single candidate.

Gelatin Plaster Casts for Anatomical Specimens.—Some time ago Mr. C. W. Cathcart, M.B. (Edinburgh Infirmary), proposed a new basis for making casts of anatomical specimens. The basis is made as follows: Take of "No. 1" gelatine, say six ounces, soak it till quite soft and swelled, afterward dry it slowly until just pliable. As it has now the minimum of water necessary, melt it in a water bath, and add six ounces (measure) of clear glycerine. When the two are thoroughly mixed, the material is ready. To render it opaque, add while it is still hot, and therefore fluid, small quantities of a thick paint made by rubbing up oxide of zinc in glycerine. When a skin color is wanted, a little vermilion is required to give a warm, life-like hue. Should other things be cast, the prevailing color can be given with water color as required (tubes of moist water color sold at two pence each will be found convenient). Several pounds of this mixture may be made at once, and portions used as required.—*Scientific American*, March 3, 1888.

The Anthropometrical Method of Identifying Criminals.—*Science* of March 30, 1888, writes that the anthropometrical method of identifying criminals, originating in Paris, has been adopted in the prison at Joliet, Ill. In addition to the photograph of the prisoner, accurate measurements of his height, the length and width of his head, the length of the left middle and little finger, of the foot, the forearm, the ear, the stretch of the arms, description of scars, color of the eyes, and so on, are recorded; and it is thus possible to identify prisoners assuming false names with far greater ease than was before possible. It is asserted that, in the two years that the system has been in operation in Paris, eight hundred and twenty-six habitual criminals arrested under assumed names have been identified. Besides the practical utility of the system, it amasses very valuable statistical data contributing toward the natural history of the criminal classes.

Death of Professor Combal.—The recent death of Combal, Professor of Clinical Medicine at Montpellier, is announced.

A Successful Removal of the Larynx.—Perossi reports in a Milan journal, *Caffè*, that there is now living in the

vicinity of Indra an Italian, named Nesmini, whose larynx was removed in 1875. This was the first extirpation of the larynx done in Italy, and the fourth in Europe. The patient continues in comfortable health.

The Monopoly of Antipyrin.—The Society of Practical Medicine, of Paris, has discussed the question of the validity of the patent which covers the manufacture of antipyrin. Their report shows that under French law it is a medicine, and its manufacture cannot be controlled by patents. Druggists have a right to fill prescriptions for antipyrin, no matter by whom manufactured, if it be a correct chemical compound.—*Le Progrès Médical*, March 3, 1888.

The Bacteriology of Beer.—Grönlund, of Copenhagen, has studied the fermentation which at times gives to beer a peculiarly bitter and disagreeable taste. He found that this process in the beer was caused by a germ known as *Saccharomyces Pastorianus* I. He advises the greatest cleanliness in the vessels used in brewing, frequent bacteriological examination of beer, and the use of only the purest yeast for manufacturing beer.—*Centralblatt für Bakteriologie*, No. 10, 1888.

The Cause of Cleopatra's Death.—Grand-Marais, of Nantes, has recently written a pamphlet to prove that the death of Cleopatra was caused, not by the bite of the asp, but by asphyxia from carbonic acid gas. He is led to this conclusion by the fact that her attendant women were found dead beside the Queen's bed, while no marks of a serpent's sting were found upon her body.—*Annales d'Hygiène*, March, 1888.

The Implantation of Teeth.—*Science* of March 23, 1888, writes that Dr. Yonger, of San Francisco, was the first dentist in this country to perform successfully the experiment of implanting teeth. This process is not to be confounded with transplanting teeth, which has been practised by dentists for many years. In the latter operation, a tooth freshly extracted is inserted in a socket from which one has just been drawn, and the parts unite, circulation between the jaw and the tooth is established, and the latter actually takes the place of its predecessor.

In Dr. Yonger's experiment, the tooth to be replaced has long been extracted, and the socket filled up with bony substance. He drills into the jaw, gouges out a new socket, and then, taking a tooth that has long been extracted, cleans it thoroughly, soaks it in bichloride of mercury, and inserts it in the socket just formed. This new tooth in due time becomes firmly anchored, and as serviceable as the original one before it became decayed. Dr. Yonger holds that the tooth is held in its place by the soft tissues surrounding it, and that the artificial socket has nothing to do with anchoring it.

The experiment described above was performed by Dr. G. M. Curtis, of Syracuse, N. Y., who afterward extracted the implanted tooth, and sent it to Dr. W. M. Gray, the microscopist of the Surgeon-General's Office, who has made a very careful examination of it. His experiments prove beyond question that the tooth so implanted is revived, the circulation is established between the socket and the implanted tooth, and that the socket does take an active part in anchoring the tooth. A tooth so implanted is much more firmly anchored in the jaw

than one of the originals, and, in the case referred to, the tooth was held so firmly that Dr. Curtis broke it in extracting it. Dr. Gray does not doubt that the soft tissues do take an active part in the operation, but he has proved his propositions in regard to the bone and the tooth beyond all question.

The Sense of Taste.—Bailey and Nichols are quoted by *Science* of March 23, 1888, in their report of recent experiments upon the sense of taste, as follows:

1. The sense of taste is vastly more delicate for bitter substances than for any others. It is possible to detect quinine in a solution that is only $\frac{1}{1000}$ th strength of a sugar solution, and we have previously shown that quinine is only $\frac{1}{10}$ th as bitter as strychnine.

2. The order of delicacy is, bitter, acid, salt, sugar, and alkali.

3. The sense of taste appears to be more delicate in women than in men. This is true in the case of all the substances excepting salt. As we had found a similar difference in favor of female observers in an earlier and independent set of experiments, which agreed in every essential particular with the results of the present test, we do not regard it as an accidental difference, or as likely to disappear in more extended investigations.

Marked differences in the delicacy of the sense of taste of different individuals were met with in the course of these experiments. There were persons who could place in the proper class, solutions containing one part of quinine in 500,000, and other substances in correspondingly high dilution, while some failed to detect solutions of more than three times the above strength. In how far this was due to education, we are unable to say. Among the men examined were many who have been accustomed to handling and recognizing drugs and medicines, and yet even these were frequently surpassed by female observers who had no such training.

Suicide by Opium in China.—A Catholic paper published in China prints a statement of a startling description with respect to opium poisoning. Opium is the general medium for suicide in China, and the remedy which is recommended, namely, to make the would-be suicide swallow the blood of a live duck as an emetic, may be passed by with a smile—although, should it act as an emetic, it may, perhaps, be as useful as any other. The statement that follows, however, is more worthy of attention, for the native paper says that there is reason to doubt if the supposed successful suicides actually die at all, as the official exhumation of many bodies shows that after the fumes have evaporated the buried persons have struggled violently to free themselves. There are many well-attested instances of persons having been buried alive, and it is generally supposed that they have been the victims of a sort of trance closely simulating death.—*Indian Medical Gazette*, January, 1888.

Removal of a University Professor for Scandal.—A professor in the University of Padua has been recently suspended from office by the Government for furthering dissension among his colleagues and diffusing scandal.

A Novel by Dr. Richardson.—Dr. Benjamin Ward Richardson has written a novel, called "The Story of a Star; a Romance of the Second Century." Its plot is laid in

the last struggle of the Jewish people for national independence. Messrs. Longman & Co., London, will shortly publish it in three volumes.

A Domestic Remedy for Ivy Poisoning.—Duffield writes to the *Scientific American* of March 24, 1888, as follows:

For many years I suffered terribly from this cause, but remembering that all poisons are acids, and that alkalies neutralize acids, I bathed the poisoned member in a strong lye made from wood ashes and obtained instant relief. Subsequently I found that the dry ashes alone, rubbed over the poisoned member, were equally effective. Since this discovery, I have had no further trouble, and having tried this simple remedy repeatedly on myself and on many others, with like good results, I am now thoroughly convinced that wood ashes will in every case prove a sure and sovereign specific for all cases of ivy poison.

Poisoning by Common Insect Powder.—A child, in Toronto, swallowed two drachms of common insect powder, and tetanic symptoms, with coma, followed. Death, however, was averted. Such symptoms as these are rare, and show that the various species of pyrethrum are not quite free from toxic properties.—*American Druggist*.

Professor Scanzoni.—The *Medical Press* writes that the celebrated professor of midwifery, of Würzburg, Scanzoni, bade last week an adieu to his professoriate. Although living for so many years in Germany, he was born in Prague in 1821, where he for some time performed the duties of clinical assistant in the Lying-in Hospital. In his valedictory lecture to the students he told them how in the fifties he had but little practice and plenty of time for literary activity. He was then summoned to Russia to attend the Empress, after which, at a bound, he became the most sought after obstetrician in Germany. Since that time his opportunities for literary work have been but few.

The Difficulties of Medical Journalism in Russia.—The *Medical Press* announces that in consequence of the confiscation of three articles by the official Censor, the appearance of the February number of the *Medizinskoe Obosrenie*, a leading Russian medical review has been delayed a month.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT U. S. ARMY, FROM MARCH 27 TO APRIL 2, 1888.

CROSBY, W. D., *First Lieutenant and Assistant Surgeon*.—Granted leave of absence for two weeks.—S. O. 29, *Department of Arizona*, March 16, 1888.

AZPELL, THOMAS F., *Captain and Assistant Surgeon* (retired).—Died March 12, 1888, at Fort Lee, N. J.

THE MEDICAL NEWS will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.

Local papers containing reports or news items should be marked. Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

All communications relating to the editorial department of the NEWS should be addressed to No. 1004 Walnut Street, Philadelphia.